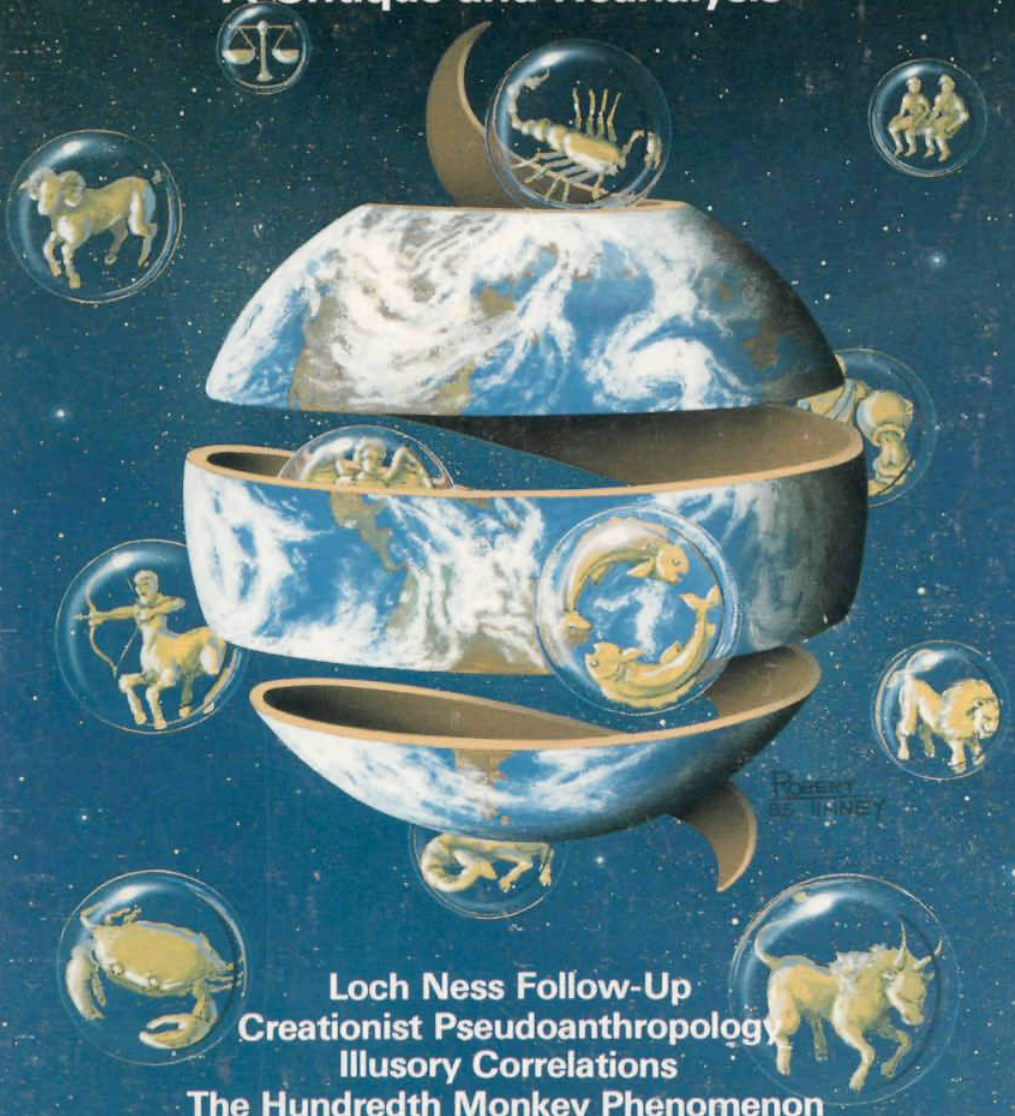
# the Skeptical Inquirer

## THE GUARDIAN ASTROLOGY STUDY
### A Critique and Reanalysis



Loch Ness Follow-Up
Creationist Pseudoanthropology
Illusory Correlations
The Hundredth Monkey Phenomenon

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# the Skeptical Inquirer

THE SKEPTICAL INQUIRER is the official journal of the Committee for the Scientific Investigation of Claims of the Paranormal.



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ON THE COVER: Painting by Robert Tinney ©1985. For more information see page 390.

# News and Comment

## The 'Green Cloud' and the Soviet UFO Scene

SOVIET SCIENTISTS are continuing to struggle with the question of UFOs, and some recent official disclosures have helped clear up a number of famous Soviet sightings of the past. But in a society as secretive as the Soviet Union, all sorts of man-made stimuli may cause massive "flying-saucer panics" and still remain classified, artificially inflating the number of unidentified flying objects in the skies of the USSR.

Another good example appeared early this year with a published account of a "green cloud" UFO that reportedly followed an airliner over the far western USSR. The description of a bright yellow light that shot a white ray to the ground and then turned into concentric cones of light seemed almost archetypical of the pseudo-UFOs seen in that region for more than a decade. The earlier ones have been traced to unannounced space-shots from the supersecret rocket center near Plesetsk, north of Moscow (*SI*, vol. 2, no. 1; vol 3. no. 2). They engendered many of the same perceptions, including spurious cases of "radar corroboration."

This latest case could well be another such event but, since the date of the encounter was not given, no correlation can be made. Since most rocket launchings from Plesetsk are made for officially nonexistent military projects, the odds of any official announcement range from slim to none.

However, a slight inkling of the truth has begun to emerge from official Soviet sources. The breach in the wall of secrecy first appeared in mid-1983 when *Pravda* published an article about UFO sightings caused by launchings from Plesetsk (*SI*, vol. 8, no. 2). This was all the more remarkable because it was the first mention of the 20-year-old missile center ever to appear in the Soviet news media.

The following year, as had been the case in 1980 and 1981 (*SI*, vol. 7, no. 1), more attention was drawn to Plesetsk when some launchings were seen from—of all places—South America. The rockets routinely fire a fourth stage after circling Earth halfway, and on March 16 and July 3, 1984, such firings left huge glowing clouds in the evening skies of Argentina and Chile. Nationwide panic followed, presumably much to the embarrassment of Soviet space-officials who refused to confess to being the cause of it all.

Late that year, two top Soviet scientists involved in the official study of "atmospheric anomalous phenomena" publicly admitted that other spectacular Soviet UFOs—such as the giant "jelly-

THE NEW YORK TIMES.

A 'Green Cloud' Adds to Soviet U.F.O. Lore

By SETH MYDANS

MOSCOW, Jan. 30 — Witnesses say it first appeared as a yellow light, then as a white ray. Then, it became a green cloud that turned into something resembling a plane.

It was the latest example of a phenomenon that has caught Russians' imagination — sightings of unidentified flying objects.

The witnesses were the passengers and crew of Aeroflot 8352 on a night flight from Rostov to Tallinn and ground controllers who had seen unexplained blips on a radar screen. And, their informant was a member of the Academy of Sciences, who solemnly declared that the sighting must have been of "what we call a U

The report, carried today in union newspaper Trud, was number that have appeared years in a nation fascinat plained phenomena such Ste Snowmen, the Bermu Loch Ness monsters and the lost city of Atlantis.

In 1967, the Soviet Air commission to investi though its findings have public. And Trud ca from a Commission of for more reports

The newspaper account emphasized the sobriety of the crew, giving the names, educational backgrounds and experience of its four members. Gennadi Lazutin during the going to say ey were being unt, the advim on a clear night w light that shot and the ground ed. as the re

Soviet Pilots Say UFO Gave Them Escort

THE ASSOCIATED PRESS

MOSCOW — Pilots and passengers aboard a Soviet airliner have reported encountering a star-like UFO they said illuminated the ground with a dazzling

fish" over Petrozavodsk in 1977, along with cases on June 14, 1980, and May 15, 1981—were due to space shots. Their impetus may well have been the circulation, in a Russian-language emigré magazine named *Our Country and the World,* published in Munich, of a detailed study of precisely those "pseudo-UFOs" and the near-comical official Soviet position of denying the reality of the rocket base. Since *Pravda* had disclosed the existence of Plesetsk a year earlier, noted scientists Vsevolod Troitskiy and Vladimir Migulin evidently felt safe in telling the truth about these incidents.

The full story, however, is unlikely to come out. It is inconceivable, for example, that the official Academy of Sciences UFO study (the so-called Gindilis Report) of 1979 will ever be repudiated, since the 1967 "flying-saucer wave" so carefully endorsed by the scientists was actually caused by fireball re-entries of Soviet space-to-earth nuclear warheads (*SI,* vol. 7, no. 3)—which Moscow had just signed an international treaty outlawing. Those "UFOs" must remain forever unidentified to protect Soviet state-secrets. Western UFO groups continue to endorse the validity of this charade for their own propagandistic purposes.

Similar restrictions surround many other space events in the USSR. (For example, the new SS-X-25 ICBM, possibly a violation of SALT-2, is tested at Plesetsk and could have been the cause of the recently reported "UFO" sighting.) Hence it should not be surprising if this Soviet UFO, as with so many others in that country, remains "unidentified"—because Moscow wants it so.

*—James E. Oberg*

*CSICOP Fellow James Oberg, author of* Red Sky in Orbit, UFOs and Outer Space Mysteries, *and other books, is an authority on the Soviet space program.*

## California Intact, Psychics Too, Despite Flubs on '84 Predictions

*At the start of each year, the Bay Area Skeptics issues a report on psychics'*

*predictions for the previous year. Here, excerpted from the January* BASIS, *the newsletter of the Bay Area Skeptics, is a condensed version of their report on predictions for 1984. As usual, it prompted much media coverage.*

THE CALIFORNIA COAST didn't drop into the Pacific Ocean, and President Reagan wasn't shot during his re-election campaign. John Glenn didn't get the Democratic nomination, and Ted Kennedy didn't elope to Europe with his secretary. These are only a few of the bizarre things that had been predicted for 1984 by famous "psychics" but failed to happen.

As in past years, the batting average of these "psychics" was near zero, reports the Bay Area Skeptics, a group dedicated to the critical examination of paranormal claims. While a few predictions did come true, especially those that are not particularly difficult to guess, and while many predictions were so vague that it is impossible to say for sure whether they came true or not, there was not a single correct prediction that was both very specific and very surprising.

For example, many psychics predicted the re-election of President Reagan, but Reagan was of course a heavy favorite. Likewise, you didn't really need psychic powers to predict that Mondale would be the Democratic nominee. Not one psychic, however, predicted the genuinely surprising events of 1984, such as the assassination of Indira Gandhi, the sudden death of Richard Burton, Gary Hart's surprising victories in the Democratic primaries, and the massive poison gas disaster in Bhopal, India.

The most dramatic prediction made for 1984 by a psychic here in the Bay Area was made by Woods Mattingley, who runs a group called "Seeker's Quest" in San Jose. Almost two years ago, in January of 1983, Mattingley confidently told a reporter for the *San Jose Mercury News* that during 1984 the California coast would drop into the Pacific Ocean. The coast was still there the last time we looked.

The famous psychic Jeane Dixon, of Washington, D.C., likewise predicted disasters for 1984 that failed to occur. This past January, she told a reporter for the *Baltimore Sun* that her vision for 1984 was too grim to talk about. "I hope you can take it, because this guy Orwell knew what he was talking about. The world isn't ready for all the things I know," said Dixon, who refused to be more specific. Her 1984 New Year's predictions published in the tabloid *The Star* were less ominous, predicting that Fidel Castro's agents would take over two small countries in Latin America, that there would be naval battles in the Caribbean between U.S.-backed and Soviet-backed forces, and that athletic scandals involving drugs would "leave the outcome of the Olympic games in doubt." Dixon also predicted that Richard Burton was "very likely" to step back into Elizabeth Taylor's life, failing somehow to foresee Burton's death during 1984.

In December 1983, San Francisco's prestigious Commonwealth Club invited local psychic Barbara Mousalam, who claims to have been 80 percent accurate

in the past, to present her predictions for 1984. Mousalam did no better than one might expect to do by guessing. She predicted an assassination attempt against President Reagan on a campaign tour during October, that the Republicans would lose control of the Senate in the election, that the draft would be reinstated, the electoral college would be abolished, the U.S.-Soviet arms agreement would be signed, Khomeini would be out of power in Iran, and a "guarded but stable peace" would be achieved in the Middle East—none of which happened during 1984. Mousalam did make one good prediction—that a woman would run for vice president on the Democratic ticket. However, political analyst Jack Anderson made the same prediction in his article in *Parade* magazine, "Headlines You Could See in 1984," indicating the degree of speculation that was already widespread concerning the possibility of a woman on the ticket. Jeane Dixon also mentioned the possibility of a woman on the ticket in April, and even named Ferraro as a likely choice, but predicted that the Democratic VP candidate would *not* be a woman this year. Jack Anderson, incidentally, had two correct predictions out of nine, a better percentage than most psychics.

Despite the miserable failure of nearly all of Barbara Mousalam's predictions for 1984, the Commonweath Club invited her back in January to prognosticate for 1985.

French psychic Marie Gadrine predicted that President Reagan would lose the November election to a handsome young "dark horse" candidate. She also predicted that Khomeini would be deposed and that a jumbo jetliner would crash into a seaside resort town. Gadrine claims to have won France's Silver Medal of Merit for "exceptional services to humanity" through her psychic counseling. No word from the French as to whether she'll have to give it back after this latest batch of failed predictions.

Dallas psychic John Catchings, who claims to have helped the police locate missing persons, predicted that a vaccine against AIDS would be developed and that Ted Kennedy would elope in Europe with a secretary in his office.

Miami psychic Micki Dahne predicted that a huge earthquake in California would unearth a major new gold vein, triggering a new gold rush, and that Mr. T would be struck by a lightning bolt attracted to his gold jewelry.

Many predictions by psychics were so vague that it is difficult to say for sure whether they came true or not. For example, Jeane Dixon's prediction that Nancy Reagan "will have one of the busiest and most trying summers of her life" is not clearly true or false. Looking only at those predictions that can be definitely evaluated and were not easy to guess (as in "unrest in the Middle East" or "the president will be re-elected"), only a very few were either correct or partially correct—about as many as you'd expect to get by guessing.

And while many predictions were very specific and very surprising, not a single prediction was very specific, very surprising—and correct. "If some psychic had said a year ago, 'Indira Gandhi will be assassinated next fall by a Sikh fanatic,' I'd have been very much impressed," said Robert Sheaffer of Bay Area Skeptics. "Or if someone had predicted that more than 1,000 people would be killed in India by a poison-gas leak in December, we'd begin to think that maybe psychic powers really do exist.

"But nobody makes accurate predictions as specific as that, no matter what they claim," said Sheaffer.

Noting that Sylvia Brown and some other psychics often charge $300 and up for private readings, Sheaffer says, "When you look at the actual track

record of predictions by these 'psychics,' it certainly appears that you don't get much for your money." If you do consult a psychic, he suggests, "insist on getting a written guarantee that you'll get your money back if the predictions fail to come true." •

## Gordon's Debunking Column Reaches 500,000 Weekly

OUR PAGES HAVE often enough criticized newspapers' handling of the paranormal. It is refreshing to report that for the past year and a half one major metropolitan daily has been publishing a weekly column critically examining paranormal claims.

Since December 1983, the *Toronto Sunday Star,* Canada's largest newspaper, has been publishing on its "Trends and Ideas" page a topical, thoughtful, well-informed column called "Debunking," written by Canadian magician/broadcaster/lecturer and CSICOP Fellow Henry Gordon. Gordon is a member of the Canadian branch of CSICOP.

"As far as I know," says Gordon, "it's the only column of its kind in a major newspaper on this continent."

The columns have covered virtually every subject of current public interest.

TRENDS AND IDEAS

DEBUNKING by Henry Gordon

A heyday for pseudo science

DEBUNKING by Henry Gordon

ESP is a problem to science

DEBUNKING by Henry Gordon

Psychics on radio shows

An early one described CSICOP and some of its tests of paranormal claims. Another contrasted the views on UFOs of James Oberg ("a strong case for the rational side") and J. Allen Hynek (whom Gordon criticized for using rhetorical questions, "commonplace among paranormalists"). It was based on Gordon's observations at the AAAS "Edges of Science" symposium (*SI,* Fall 1984). Another explained why, despite the "prestigious background" of British psychologist H. J. Eysenck, Gordon finds his writings about the paranormal naive and misleading.

While most newspapers were publishing stories promoting the sensational claims of psi advocates Russell Targ and Keith Harary (*Mind Race*), Gordon's column was providing clear and cogent criticism. Another reported on fortune tellers convicted of bilking victims of amounts from $200 to $5,000, noted that his files contain almost identical stories going back decades, and went on to explain how the powerful desire to believe in comforting fantasies allows the clever psychic to take advantage of victims. The same column related Gordon's experience after an appearance at a television station in Baltimore confronting several "psychics." A young woman told him, "I really enjoyed your presentation—I agree with you 100 percent." Then she quickly added, ". . . because I think these psychics are aided and abetted by evil spirits." Fear and superstition, noted Gordon, are the charlatans' allies.

Recent columns have discussed the special problem posed by "psychics" on radio shows, the methods of fraudulent psychics, and the issue of personal experiences that people interpret as ESP. A January column was devoted entirely to the work of CSICOP and the SKEPTICAL INQUIRER.

To introduce Gordon's column as a regular Sunday feature, editor Gerry Hall published a lengthy "Editor's

Notebook" column touting Gordon's abilities as a magician to see through psychic trickery and approving of his call for controlled scientific investigations of psychics' claims. "Henry Gordon is perhaps the best man in Canada to scrutinize the world of the unexplained," wrote Hall. "He knows every psychic trick in the book."

The editor's column was headlined "We Need a Little Dash of Debunking."

Gordon says he's been getting good mail-response to his columns and is pleased the scientific perspective on paranormal claims is being spread to an important part of Canada and bordering cities in the United States.

—*Kendrick Frazier*

## REASON in West Virginia: An Eventful First Year

SPRING 1985 brought the first anniversary of the formation of the Committee for Research, Education and Science Over Nonsense (REASON), in Huntington, West Virginia.

Sponsored by the Department of Psychology at Marshall University, REASON advocates quality science education and a skeptical approach toward claims of the paranormal, and draws its members from both the university and the surrounding community. It has become a resource for the local media on scientific, pseudoscientific, and paranormal topics, and at present incorporates a Scientific and Technical Board, divisions for Psychic Claims Testing and Paranormal Health Claims, and a general membership.

In its first year, REASON sponsored a series of educational programs, with films, panels, and speakers on topics like the Tina Resch case, the recent controversial NOVA film on remote viewing, and the writings of Erich von Däniken. Speakers and demonstrations were provided for civic groups and schools.

Research by REASON members included debunking studies of astrology, biorhythms, and the "unusual" similarities between identical twins (see "Similarities Between Identical Twins and Between Unrelated People," W. Joseph Wyatt, et al., *SI*, Fall 1984). There was a measure of national attention and considerable local interest when REASON offered a $1,000 prize for the demonstration of a paranormal phenomenon or ability under controlled conditions. The prize remains unwon.

REASON begins its second year optimistically, with a brand-new newsletter, a sizable dues-paying membership, and better scientific and educational resources. Inquiries about the organization, its activities, or its newsletter may be directed to: Steven Cody, Chairperson, REASON, Department of Psychology, Marshall University, Huntington, WV 25701.

—*Steven Cody*

## Bay Area Group Opposes Licensing Psychics

*The following resolution was prepared and issued by the Bay Area Skeptics in response to a proposed ordinance by the City Council of Concord, California, to license "fortune tellers" and "psychics."*

BAY AREA SKEPTICS
RESOLUTION REGARDING
THE LICENSING OF "FORTUNE TELLERS"
ADOPTED UNANIMOUSLY BY THE BOARD
JANUARY 26, 1985

Bay Area Skeptics opposes the licensing of "fortune tellers" and "psychics." Such licensing legitimizes activities of those whose claims have never been demonstrated to be valid. Furthermore, it

would be impossible for the City of Concord (or any other governmental agency) to regulate these activities, as there are no standards or criteria by which they could be judged. Bay Area Skeptics and other organizations, such as the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), as well as the general scientific community, have for many years investigated psychic claims. The conclusion is that there is no credible evidence that these claims are valid. We can provide material, including books and learned periodicals, to back up these statements.

We oppose legitimizing so-called psychic and fortune-telling activities. We consider the irrational appeals of such claims to be harmful to society. The survival of a democratic society depends upon the ability of its citizens, especially those entrusted with the responsibility to make and interpret laws, to determine the difference between sense and nonsense.

## Baugh's Dinosaur: Paleontology on the Paluxy

WHEN LAST WE saw the Reverend Carl Baugh, that intrepid Bible-scientist was at the 1984 National Bible-Science Conference in Cleveland, proclaiming that two rock concretions were the fossilized skulls of a cat and a primate. More recently he has been busy at his diggings on the Paluxy River near Glen Rose, Texas, where he claims to find human and dinosaur footprints in the same rock strata. And last summer, according to the December 1984 *Bible-Science Newsletter,* Baugh "discovered an actual dinosaur skeleton *with some parts still not fossilized"* (italics in original).

Baugh called the bones a "fairly complete dinosaur skeleton." They were found on the ranch of Al Parker, about two miles from Baugh's primary excavation site. Parker reported on August 16 that "about 90 percent of the skeleton has been uncovered, including a 38-inch-long leg bone and six claws." Baugh confidently identified the contiguous sequence of vertebrae found in the rock as the neck of a sauropod (plant-eating) dinosaur. He was ecstatic about the claws, which he considered so valuable that he put them in a safe-deposit box.

Well (skeptics be damned), Baugh really *did* find dinosaur remains. Wann Langston, Jr., a paleontologist with the University of Texas, visited the site and verified that fact. As for the details, however, Langston found that Baugh's analysis of his discovery was wrong from start to finish. What Baugh identified as the neck of a sauropod was actually the tail of a carnosaur. What Baugh identified as a leg bone was an ischium, part of the pelvic arch. Baugh's unfossilized "dinosaur claws" were actually fossilized goniopholid crocodile teeth! To show his versatility, Baugh was also showing people a mammoth tooth and calling it a fossil from a sabertooth tiger.

A few months after the initial discovery, Baugh announced that he had found another dinosaur a few feet away. (Langston suspects that Baugh actually found more bones from the same animal.) Soon afterward, a Texas "gully-washer" raised the Paluxy River enough to flood the site and wash everything away. It's not clear what bones Baugh had recovered, or in what condition.

The whole story would be hilarious except that Baugh's dinosaur was almost certainly important. While Langston could not positively identify the animal from the few bones he saw at the site, he suspects that they belonged to an *Acrocanthrosaurus,* a genus known only from a few partial skeletons. Baugh's excavation and preservation methods leave much to be desired. (He had the

two small rocks he thought were important fossilized cat and primate skulls cut in half with a rock saw, which took out a quarter-inch kerf!) It appears that an important paleontological find has been lost to science.

—*Robert Schadewald*

*Robert Schadewald keeps close watch on the creationist scene.*

## Time-Life's Supernatural Series

*The following letter was sent to Time-Life Books by* SI *reader Jeremy Larner, who has given us permission to reprint it here.*

Dear Sirs:

When my children were growing up, I subscribed to your Science and Nature series, and found myself impressed and gratified to see the scrupulosity and care with which your writers and editors marshaled a vast amount of scientific information. It made me proud to have once been a free-lance writer for *Life* magazine. I am sure your publications helped my children to form a more accurate impression of the world around them.

Imagine my dismay, then, to see your advertisements on television for your new series on the supernatural. Your ads make it seem as if claims of psychic powers are a matter of scientific debate and might even be scientifically verifiable. People of your sophistication surely know that no one has ever verified the kind of feats you depict in your advertisements. I can only conclude you are cynically exploiting the public, most of which unfortunately does not know the difference between real and pseudo-science.

It is particularly unfortunate to imagine the effects this approach may have on the minds of children. To me, it represents a total departure from the previous integrity of your series.

## A Pox on Prognosticators

DO YOU GET asked, "What harm can going to a fortune teller do?"

Ann Landers's nationally syndicated newspaper column for November 15, 1984, had a good answer. A reader wrote that a friend had persuaded her to go to a "trance medium" who would recite incidents from her past and present and also predict the future. The woman went to the medium, paid her $40, and went through the session. At the end, she realized the medium had said nothing about her having any children in her life. She was a newlywed and wanted a family. The medium informed her she saw no children in her future.

"Ann, I was devastated," the reader wrote. "I cried all the way home and lived with a kind of aching sadness for the next year."

The story has a happy ending. "Thank God, I now have a happy, healthy, wonderful 8-month-old son. But believe me, my pregnancy was filled with anxiety and fear. For heaven's sake, if there is anyone out there who is tempted to go to a fortune teller, save yourself the money, needless worry, and pain. What will be, will be."

She signed herself, "A Pox on All Prognosticators."

"Those gazers are awfully good guessers," Landers responded. "Thanks for shattering their crystal balls."

—*K.F.*

## Silly Season

IN ITS JANUARY 2, 1985, edition, the syndicated TV show "PM Maga-

zine" took a refreshing approach to the flurry of psychic predictions in the supermarket tabloids. It ignored the new ones and instead took a retrospective look at the predictions the *National Enquirer* had published a year earlier for 1984.

The tabloid had made 54 predictions. The scorecard, said the TV hosts, came out this way: 2 right (drops in inflation and unemployment), 2 partly right, 38 wrong, and 12 "just plain silly."

—*K.F.*

## Darts, Anyone?

AS PART OF its coverage of 1984 events at year's end, NBC-TV presented staffer Mike Jensen reporting the results of an experiment begun in 1980. He and economist-publisher Malcolm Forbes had each tossed darts at the stock page of the *Wall Street Journal*, then theoretically bought stocks in response to that random selection. Had he really invested, Forbes would have come out ahead by far. The Dow-Jones overall figure for the four-year period of the experiment was a 19-percent increase, but Forbes's darts came up with stocks that soared 119 percent! As for reporter Jensen, perhaps he is eyeing Forbes's job and has a very slightly better throwing arm. He beat the Dow-Jones too, with a 120-percent increase in his choice of stocks.

## UFO Federation Falls on Hard Times

ADVANCE PUBLICITY for the 1982 annual conference of the Mutual UFO Network (MUFON), held in Toronto, touted the meeting as a "Summit Conference" for leaders of the many diverse UFO organizations in the United States and Canada. For the world of UFOlogy, it could be as important as the Constitutional Convention held two centuries earlier in the United States. After all, the leaders of most of the UFO groups espoused the same dogma: that UFOs almost certainly are extraterrestrial craft and that the U.S. government (if not all governments) is engaged in a gigantic conspiracy to withhold the truth about UFOs from the public. In unity there could be sorely needed strength.

UFO leaders who journeyed to Toronto agreed to create a federation to speed the achievement of their oft-stated common goal: to solve the UFO mystery. They agreed to form the North American UFO Federation (NAUFOF), and a steering committee was named to prepare its bylaws.

At the next year's annual MUFON conference in Pasadena, on July 3, 1983, a nine-person board of directors for NAUFOF was elected, representing nearly as many different UFO organizations. Board members included Walt Andrus, international director of

MUFON; J. Allen Hynek, head of the Center for UFO Studies (CUFOS); and Richard F. Haines and Richard Hall, representing the Fund for UFO Research. Hall earlier was deputy director of the now-defunct National Investigating Committee of Aerial Phenomena (NICAP) when it was the nation's largest UFO group.

According to the new bylaws, NAUFOF's objectives included "to unite UFO organizations in North America in a format of understanding, cooperation, and harmony of efforts to study and resolve the UFO phenomenon" and "to share information on UFO sightings and analyses."

MUFON's Andrus hailed the creation of NAUFOF as "a giant step forward in securing cooperation between UFO groups in North America and presenting a united front in UFOlogy to the World." As reported in the July 1983 *MUFON UFO Journal*, the new federation "will work directly with the worldwide International Committee on UFO Research"—then slated to meet the next month in England—which had similar objectives for global UFOlogy. One UFO group was notable in its absence from NAUFOF—the Aerial Phenomenon Research Organization (APRO). It was the oldest UFO group in the United States, and Andrus had been one of its most active members until he split with its founders, Mr. and Mrs. James L. Lorenzen, to create his own organization.

In the September 1983 MUFON journal, Andrus discussed a tiny cloud on NAUFOF's glowing horizon—how to finance its operations. The obvious solution was to ask member UFO-groups to contribute funds, but Andrus indicated that MUFON (the nation's largest) could ill afford to do this without increasing its membership dues, which he was not eager to do.

MUFON members heard nothing more of consequence about NAUFOF for more than a year. Then, in the December 1984 MUFON journal, Andrus revealed the bad news to MUFON's 1,300 members in the following words:

> On July 7, 1984, Dr. J. Allen Hynek, John Timmerman [also of CUFOS], Richard H. Hall, John F. Schuessler [a MUFON official] and Walt Andrus met in San Antonio to discuss the potential future of the North American UFO Federation (NAUFOF) as it concerned CUFOS, MUFON, and the Fund for UFO Research. The Board of Directors of CUFOS had already made a decision not to support NAUFOF and Dr. J. Allen Hynek resigned from the NAUFOF Board. With the resignation of Dr. Richard F. Haines as Director [of NAUFOF] in September, 1984, the future looked very bleak considering who would automatically ascend to the directorship. [The person was not identified.]
>
> MUFON, SKYNET and the Fund for UFO Research were the only organizations who paid membership dues in 1983-84. After due consideration, MUFON has elected not to provide financial support to the NAUFOF for 1984-85 since it has deteriorated to an association of UFO splinter groups that are not interested in cooperation except on their own terms. Such an organization is not qualified to represent the North American Continent in international UFO circles.
>
> Therefore Richard H. Hall and Walt Andrus proposed that the NAUFOF be dissolved through a vote of the Board of Directors. Insufficient votes were cast for the dissolution of the Federation. MUFON and CUFOS are already member organizations of the International Committee for UFO Research (ICUR), therefore our financial and research activities will be directed to this prestigious international body.

While MUFON's members did not learn of NAUFOF's problems until late 1984, six months earlier the word had

reached UFOlogists lucky enough to be on the mailing list of an irreverent monthly newsletter called *Saucer Smear.* It is published by longtime UFOlogist-humorist James W. Moseley, whom Andrus refuses to accept as a member of MUFON.

In early August, Moseley's newsletter reported that Haines was "stepping down" as NAUFOF's leader, that Hynek did not want the position, and that it would most likely go to Peter Mazzola, the federation's deputy director. Mazzola and his Scientific Bureau of Investigation (SBI) are comparative newcomers in the world of UFOlogy. Moseley reported that "a vote by mail is now in process among Board members as to whether or not it is worthwhile continuing this well-intentioned but largely unsuccessful organization."

—*Philip J. Klass*

*Philip J. Klass is author of* UFOs: The Public Deceived *and chairman of CSICOP's UFO Subcommittee.*

## Astrology Disclaimer Update

THE *PITTSBURGH POST GAZETTE* has joined the list of newspapers whose editors have agreed to publish CSICOP's suggested disclaimer with their astrology columns

## Additions to The CSICOP Roster

WE ARE PLEASED to announce that the CSICOP Executive Council recently elected Murray Gell-Mann, Stephen Toulmin, Andrew Fraknoi, David Marks, Henry Gordon, Robert Steiner, Lee Nisbet, and John Cole as Fellows of the Committee. Murray Gell-Mann is a Nobel Prize laureate and professor of physics at the California Institute of Technology. Stephen Toulmin is professor of social thought and philosophy at the University of Chicago. Andrew Fraknoi, a CSICOP scientific consultant since 1981, is the executive officer of the Astronomical Society of the Pacific. David Marks, head of the New Zealand branch of CSICOP and a former CSICOP scientific consultant, is temporarily professor of psychology at University College London. Henry Gordon, a magician and newspaper columnist in Toronto, has been a technical consultant to CSICOP for many years. Robert Steiner is a magician and a longtime CSICOP technical consultant.

Lee Nisbet, associate professor of philosophy at Medaille College and executive director of the Committee since its founding, has been appointed special projects director. John Cole has taken the position of full-time executive director; he is an anthropologist and has been a CSICOP scientific consultant since 1978.

The Council has also elected the following scientific or technical consultants: Richard Busch, a magician in Pittsburgh; Steven D. Schafersman, a geologist at Rice University in Houston, and Steven N. Shore, an astronomer at the Space Telescope Science Institute in Baltimore.

*Paul Kurtz*
*CSICOP Chairman*

(see *SI*, Spring 1985, pp. 194-195). In the meantime, the newspapers that have printed CSICOP's request as a letter to the editor, published stories about it by their own staffers, and/or carried the article on the scientific evidence for astrology by Paul Kurtz and Andrew Fraknoi now number in the hundreds.

—*Andrea Szalanski*

## On the Record: Astrology and Cosmobiology

"To us, the Gauquelin correlations make no physical sense and are inconsistent with the body of natural empirical evidence. The correlations, are weak, and it is not clear that they could not be spurious. In any case, the implication of a causal relationship is not justified on purely statistical grounds. There seems not a hint of a reasonable explanation for them. The successful replications are essentially only those by Gauquelin; Eysenck omits reference to the several careful studies that failed to confirm the effect. . . . Eysenck refers to the work of the Gauquelins as a 'nugget of gold' amidst the rubbish of evidential support for astrology. . . . We will be very surprised if the golden nugget does not turn out to be iron pyrite."

—Astronomers Philip A. Ianna and Roger B. Culver, in *Nature*, Feb. 7, 1985, replying to a review by H. J. Eysenck

*There's just no call for paperclip twisting. When you get to spoon bending, we'll talk. Okay?*

# The CSICOP 1985 International Conference

*Sponsored by the Committee for the Scientific Investigation of Claims of the Paranormal in cooperation with the British Branch of CSICOP, Inc. at*

**University College London**
**Friday and Saturday, June 28 and 29, 1985**

## "INVESTIGATION AND BELIEF"

### Friday

**2:30-6:00 P.M.**

**Introduction: "Skepticism and the Paranormal,"** Paul Kurtz, CSICOP Chairman

**Moderator:** James Alcock, psychologist, York University, Toronto

**"UFOlogy: Past, Present and Future,"** Philip J. Klass, science writer

**"Past Lives Remembered,"** Melvin Harris, researcher and broadcaster

**"Age of Aquarius: Waiting for the Dawn,"** Jeremy Cherfas, Consultant, *New Scientist*

**7:30 P.M.**

**Banquet** (By invitation only.)

### Saturday

**9:45 A.M.-1:00 P.M.**

**Moderator:** Christopher Scott, statistician, psychologist

**"Parapsychology: A Flawed Science,"** Ray Hyman, psychologist, University of Oregon

**"Fallacy, Fact and Fraud in Parapsychology,"** C. E. M. Hansel, psychologist, University of Wales

**"The Columbus Poltergeist,"** James Randi, conjurer and author

**2:30-6:00 P.M.**

**Moderator:** Ken Frazier, Editor, SKEPTICAL INQUIRER

**"Why People Believe,"** David Marks, psychologist, University College London

**"The Psychopathology of Fringe Medicine,"** Karl Sabbagh, journalist

**"A Realistic View"** (Blindfold Demonstration) David Berglas, television's "Man of Mystery"

**Place:** University College London
Chemistry Department
Gordon Street
London WC1, England.

**Fee:** £12
$15 U.S.

For further details or to register, please contact Michael Hutchinson, 10 Crescent View, Loughton, Essex 1G10 4PZ, England (Tele.: 011-44-1-508-2989). In North America, contact Mary Rose Hays, CSICOP, Inc., P. O. Box 229, Buffalo, NY 14215 (Tele.: 716-834-3222).

MARTIN GARDNER

# Notes of a Psi-Watcher

## Welcome to the Debunking Club

WHEN DEFENDERS of a pseudoscience want to put down critics, they like to call them "debunkers." The implication is that debunkers are not open-minded skeptics, eager to learn the truth. Instead of rational arguments, critics of skepticism rely mainly on ridicule and name-calling. Should we skeptics take offense when we are accused of debunking?

The origin of "bunk" is amusing. I happen to live a few miles from Buncombe County, in the mountains of western North Carolina. Back in 1820 the county's representative in the U.S. Congress had a habit of putting colleagues to sleep with long speeches "for Buncombe." Shortened to "bunkum" or "bunk," the word became a synonym for political claptrap. In today's dictionaries *bunk* is defined as nonsense, and *debunking* as the exposing of sham or falsehood. Who could object to that? Nevertheless, it may be that *debunk* is becoming a term of reproach, like the old word *muckraker,* now replaced by the more dignified "investigative reporter."

Stephen Jay Gould is among the many top scientists who do not mind being called debunkers. His splendid book *The Mismeasure of Man*—an attack on historic efforts to link intelligence to race, sex, or the shape of the head—has a section headed "Debunking as a positive science." Gould sees debunking not only as admirable but as essential to the health of science.

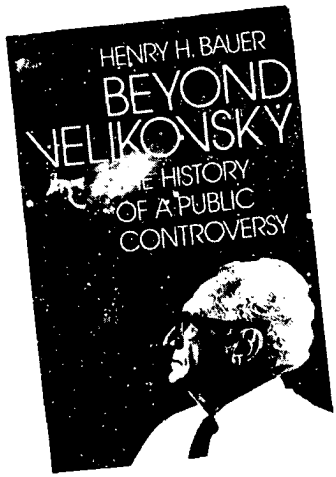
Have you noticed that believers in a pseudoscience are all in favor of debunking pseudosciences in which they *don't* believe? Indeed, many times they themselves ridicule believers of other doctrines. Frequently, when they reply to their own detractors, they hurl insults of the very sort they condemn their critics for using.

The thoughts above circulated through my brain while I was reading *Beyond Velikovsky,* by Henry H. Bauer, newly published by the University of Illinois Press (1985, 354 pp., $21.95). A Vienna-born chemist, Bauer is dean of the College of Arts and Sciences at Virginia Polytechnic Institute and the author of several technical books. "Many scientists derisively attacked Velikovsky's theories . . . ," says the book's jacket. "But they seriously undercut their case by resorting to innuendo, ridicule, misrepresentation, *ad hominem* arguments. . . ." In brief, they were debunkers of the worst sort. Because these attackers of V (as I shall henceforth refer to Velikovsky) include such distinguished scientists as Carl

Sagan and Gould, science writers like myself, Isaac Asimov, L. Sprague de Camp, and other friends, I opened the book with understandable interest. I expected to find a careful, dispassionate evaluation of everyone involved in the V controversy. To my surprise, I found Bauer writing exactly as he accuses debunkers of doing.

Some samples: V was "an ignoramus masquerading as a sage" (p. 94). He was "quite ignorant of science, and I cheerfully dismiss as nonsense his *explanations* of physical events, where indeed he gives any" (p. 319). V was "unaware of contemporary cosmological views, even to the extent of being unclear about the distinction between the problem of the age and origin of the universe and that of the age and origin of the solar system" (p. 129). "There is warrant to describe him as a pseudo-scientist" and his "ideas about matters of natural science are not worth taking seriously" (p. 133). V is "an arch-dogmatist who regards his own intuition as a more valid guide than all the accumulated body of fact and theory. . . ." (p. 122).

Bauer accuses V of having been a man without humor who saw himself as a heretic and genius in the company of Maxwell and Einstein and who took every opposition to his views as evidence of a vast conspiracy by the establishment. In plain language, V was paranoid. "He misrepresents theories and facts extant at the time he wrote—and not because he had not read about them, for he quotes from authoritative sources: either he did not understand, or he deliberately misrepresented. . . . He uses the jargon of science as though he understands it. His tone is that of one who is discussing subjects with which he is familiar, yet a close look at what he says reveals that he is not competent to carry on such discussions" (p. 121). Bauer is here commenting on V's little-known *Cosmos Without Gravitation,* in which he defends the view that gravity is electromagnetic. On the basis of this book alone, Bauer writes, "I would not hesitate to characterize the author as a crank or a charlatan" (p. 121).




Over and over again Bauer reminds his readers that because V is a crank it doesn't follow that his views are "wrong." What Bauer means is that one can never say with absolute certainty that any theory is right or wrong. Who could disagree? I know of no philosopher who doesn't take this commonplace view for granted. This is not to say that theories can't be evaluated, some with extremely high degrees of probability. Bauer himself often states flatly that V is "downright wrong" and "quite wrong." In this connection it is interesting to note that when Gould debunks V's major geological blunders (in *Ever Since Darwin*) he writes: "Velikovsky is neither crank nor charlatan—although . . . he is at least gloriously wrong."

Naturally I agree with everything Bauer has to say about V in his book and in his excellent review of Alfred de Grazia's *Cosmic Heretics* in the Spring 1985 SKEPTICAL INQUIRER. But the funny thing is that Bauer is as harsh on V's detractors as he is on V. They are

constantly accused of "gross ineptness," yet his own summary of why he decided V was a crank says very little that V's debunkers did not say in more detail and with more cogent arguments. For reasons hard to fathom, Bauer is as eager to bat V's critics on the head as he is to bat V. His technique, borrowed from the Velikovskians, is twofold: (1) say little about the strongest criticism—Gould's essay "Velikovsky in Collision," for instance, is never mentioned, except in the book's excellent bibliography; and (2) nitpick.

A fine specimen of nitpicking is Bauer's attack on Sagan. In his famous lecture on V at a meeting of the American Association for the Advancement of Science, Sagan discusses in detail ten of V's greatest howlers. Bauer ignores all of these criticisms except the one in which Sagan, according to Bauer and others, made a dubious calculation of probability. I come off a trifle better than most debunkers because Bauer credits me with being the first to point out V's enormous debt to Ignatius Donnelly's crank work *Ragnarok,* but other science-writers are denigrated in terms as strong as any that Bauer applies to V.

In some cases, Bauer seriously misinterprets. He quotes Asimov: "Velikovsky doesn't accept the laws of motion, the law of conservation of angular momentum, the law of conservation of energy and other such trivialities." Bauer calls this sentence irresponsible, inaccurate, and misleading (p. 141) on the grounds that V did not deny such laws. Now surely it is obvious that Asimov never for a moment thought that V did not accept *some* laws about energy, motion, and momentum. What he clearly meant was that V's wild celestial scenarios violate all classic laws. In other words, V did not accept the laws on which all scientists agree. He accepted only the curious laws he himself invented, laws totally unsupported by evidence, serving no purpose except to bolster V's bizarre interpretations of legends in the Old Testament and elsewhere.

To justify calling V a crank, Bauer acknowledges that he has no objection to anyone calling *him* (Bauer) a crank. Why would anyone do that? Because, he confesses, he firmly believes in the reality of Loch Ness monsters! At first I thought he was joking, but no—he is deadly serious. The book is dedicated to his friend Tim Dinsdale, whose popular books on water monsters Bauer greatly admires. He is convinced that Dinsdale's 1960 film showing a dark spot moving through the loch's waters is an authentic film of the creature's hump. Recalling Bauer's bibliography of references on the Loch Ness monster in *Zetetic Scholar* (No. 7, 1980), I checked it to see what he had to say about this dreary literature. Sure enough, Bauer gives high marks to the two books on sea serpents that Rupert Gould, a British naval officer, wrote in the early 1930s. "Gould's perspicacity is evidenced by his conclusions, borne out by the decades of further investigations," Bauer comments. "Nessie is up to 45 feet long . . . and is a land-locked sea-serpent." Books and articles critical of sea-serpents are either ridiculed or considered unworthy of citing.

Bauer's present opinion is unchanged. On page 139 of his V book he declares: ". . . Just as from personal inquiry I believe Velikovsky to be a pseudo-scientist, so also on the basis of my personal inquiries I believe in the existence of Loch Ness monsters ("Nessies") and of sea serpents. In Loch Ness there is a breeding population of large aquatic animals with powerful flippers, long thin necks, and bulky humped bodies, animals not as yet known to 'science.' I believe that because I myself have examined the evidence of eyewitnesses, of photographs, of sonar observations. So,

someone who lumps my Nessies together with the case of Velikovsky loses credibility in my eyes; it indicates to me that he probably takes his opinions at second hand or after only cursory reading."

A footnote on the same page, referring to sonar results and underwater photos, was written before publication in the SKEPTICAL INQUIRER (Winter 1984-85) of "Sonar and Photographic Searches for the Loch Ness Monster: A Reassessment," by sonar specialists Rikki Razdan and Alan Kielar. In 1983, after finding the evidence for Nessie to be near zero, the authors made a careful sonar monitoring of Loch Ness. It lasted seven weeks, searching to a depth of 33 meters in an area where previous contacts had been reported. They found nothing larger than a one-meter fish. It will be interesting to see what Bauer has to say about this in his next book. According to the dust jacket of his book on V, it will be a study of the Loch Ness controversy.

It goes without saying that good debunkers should do their best to understand what they debunk and to avoid as many mistakes as possible. Unfortunately, life is short, scientists are busy, and cranks have a habit of writing lots of books and articles. You can hardly blame scientists for not spending years digging up everything a crank has published, not to mention unprinted manuscripts available only to the master's acolytes.

When V announced that all the craters on the moon were formed a few thousand years ago by the bubbling of its molten surface, can you blame astronomers for not trying to give the public and V's followers a course on ancient lunar impact craters? When V attributed earth's petroleum to vermin flourishing on Jupiter and being carried to the earth's atmosphere by Venus, can you blame geologists for not regarding this as worthy of a detailed rebuttal? Judging from past history, such attempts to enlighten Velikovskians would have been like trying to write on water. There are times when the claims of a crank are so preposterous, such obvious examples of pure bunk, that ridicule is unavoidable. Welcome, professor Bauer, to the V debunking club. •

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## CSICOP 1986 Conference in Colorado

CSICOP's 1986 meeting is slated for April 25-27 at the University of Colorado at Boulder. It will be cosponsored by CO-RAP and the University of Colorado's Committee on the History and Philosophy of Science.

The following topics are tentatively scheduled for discussion: Reincarnation and Life After Life; Quantum Physics and Parapsychology; The Condon Report—16 Years Later; The Sociology of Skepticism; and An Update on ESP.

Plans are to include informal roundtable discussions on various topics and a meeting of current and potential affiliates. Watch for details in future issues of SKEPTICAL INQUIRER.

# Audiotapes of CSICOP's 1984 Conference Are Now Available!

If you couldn't attend "Paranormal Beliefs: Scientific Facts and Fictions," November 9 and 10, 1984, at Stanford University in California, you can now listen to the proceedings on tape.

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ROBERT SHEAFFER

# Psychic Vibrations

WE HOPE YOU got your entry in before the deadline. The Academy of Religion and Psychical Research has announced its annual Robert M. Ashby Memorial Award. Each year, a prize of $250 is awarded for the best paper on "The God/Psi Connection: Interaction/Merging of Mystical and Psychical Experience." If you somehow missed this opportunity, you can submit an entry for next year's award. However, if writing papers seems too tame, you can apply to the Arthur Ford International Academy of Mediumship in Roswell, Georgia, to see if you qualify for its "Trance Mediumship Internship." Those fortunate enough to be accepted as interns will receive their training from no less an authority on spiritualism than the late Arthur Ford himself. While Ford's abilities at communicating with us from the beyond must be at least as prodigious as those he displayed while on earth communicating in the other direction, he uses, nonetheless, an earthly medium as his channel for instruction.

* * * * *

The *Bulletin of the Tychonian Society*, published by a group of "creationist extremists" who accept the geocentric hypothesis primarily on religious grounds, reports that an attempt will be made to place an experiment aboard a future Space Shuttle flight to prove their contention that the earth is stationary, with the sun circling around it. As interpreted by geocentrists, the famous Michelson-Morley interferometry experiment, which failed to discover the expected "ether drift" due to the earth's orbital motion, is consistent with two competing hypotheses: that the speed of light is always a constant to

observers in any frame of reference (Einstein's), and that the earth is indeed stationary (theirs). Therefore, they propose to repeat the experiment in the Space Shuttle, which they would agree is definitely moving. They expect to discover that light rays traveling in the direction of the spacecraft's motion will move faster than those going in the other direction. This result, if obtained, would come as a surprise to virtually every physicist on earth. One of their members, Martin Sanderse, has entered such a proposal in the Canadian SPAR Aerospace Contest, to be executed as a Category III experiment aboard the Shuttle. Future developments, if any, will be reported. One outspoken Tychonian, James Hanson, has listed more than 2,000 Bible verses that are geocentric but reports that he has "yet to come on one verse that is even remotely heliocentric."

Elsewhere in creationism, the *Bible Science Association Newsletter* reports on a new book by Dr. Thomas Barnes, titled *Physics of the Future,* that it is offering for sale. The book attempts to "unify" the findings of modern physics with those of classical dynamics. Unfortunately, the greatest impediment to this unification "has been the acceptance of Einstein's Special and General Theories of Relativity." To remedy this problem, Dr. Barnes proposes a model in which an electric field is "fed back" from a moving particle to the initial field of the charge, producing the effects that relativistic physicists have been misinterpreting as changes in length and time. In this model, a neutron is viewed as being made up of an electron and a proton spinning madly, aligned so that their magnetic repulsion is just enough to balance their electromagnetic attraction. If this is true, the electron must be spinning so rapidly that its surface is moving faster than the speed of light. However, having repudiated Einstein's laws, this problem does not seem insurmountable. The reviewer suggests that "Dr. Barnes will take his place alongside Newton, Faraday, Gauss, and Maxwell," presumably replacing somebody like Einstein, Planck, or Bohr, whose work has been superseded.

* * * * *

Since it is so rare nowadays for anyone to have an opportunity to actually encounter "the demonic," the following account seems especially valuable in giving us insights into that ghastly realm: "When the demonic finally spoke clearly in one case, an expression appeared on the patient's face that could be described only as Satanic. It was an incredibly contemptuous grin of utter hostile malevolence. I have spent many hours before a mirror trying to imitate it without the slightest success." So reports M. Scott Peck in his book *People of the Lie,* which was excerpted in *Fate* magazine. What is remarkable is that Peck is not a clergyman or medium but a psychiatrist, who sometimes resorts to exorcism in those cases

where he believes the subject's problems are of supernatural origin. (A better term, he suggests, would be *subnatural.*) In another patient, "the demonic" fleetingly revealed itself "with a still more ghastly expression. The patient suddenly resembled a writhing snake of great strength, viciously attempting to bite the team members." Even more frightening, however, than the patient's serpentine behavior was the expression on his face: "The eyes were hooded with lazy reptilian torpor—except when the reptile darted out in attack, at which moment the eyes would open wide with blazing hatred. Despite these frequent darting moments, what upset me the most was the extraordinary sense of a 50-million-year-old heaviness I received from this serpentine being."

Lest anyone think that an exorcism might be a nifty thing to do at one's next session with the analyst, Dr. Peck warns that "Satan does not easily let go. After its expulsion, it seems to hang around, desperately trying to get back in." As a "hardheaded scientist—which I assume myself to be," Peck feels he is able to explain 95 percent of the incidents involved in these two cases in terms of conventional psychiatric theory—catharsis, marathon group therapy, deprogramming, etc. But that still leaves, he says, a critical 5 percent without a normal explanation. Since this same "signal in the noise" argument is heard about many other unsubstantiated claims—UFOs, monster sightings, "psychic" events, etc.—perhaps the scientific study of demons should take its rightful place among these other frequently lauded "infant sciences." •

# The Guardian Astrology Study: A Critique and Reanalysis

*Last year a massive study of sun sign and occupations was published, and some astrological effects were claimed. A reanalysis shows the results can be explained by statistical fluctuations and self-attribution.*

Geoffrey A. Dean, I. W. Kelly, James Rotton, and D. H. Saklofske

IN A FOUR-PART series from March 19 to March 22, 1984, the *Guardian*, the prestigious British newspaper, presented the results of a massive study of the relationship between occupation and sun sign. The analysis was conducted by Professor Alan Smithers, head of the Department of Education at Manchester University. As Smithers (1984a) tells us:

> Every ten years we in Britain are all obliged to take part in the national census. This includes questions on occupation and date of birth. A computer analysis of the responses should show quite conclusively, at least for one country at one point in time, whether there is any connection.
>
> With the help of the Koestler Foundation and the Guardian, it has been possible to purchase from the Office of Population Censuses and Surveys a special tabulation of information collected in the 1971 Census, the latest one for which the data we wanted are currently available.
>
> The office was able to provide a one in ten sample of all "economically

*Geoffrey Dean, formerly a research scientist with the Commonwealth Scientific and Industrial Research Organization, is a technical editor with Australia's major publisher of technical documentation. Ivan Kelly is an associate professor of educational psychology at the University of Saskatchewan. James Rotton is an associate professor of psychology at Florida International University. Don Saklofske is an associate professor of psychology at the University of Saskatchewan.*

active persons," which includes those in employment, those who were sick at the time of the census, and those available for work but who did not have a job. Only those born in Britain and usually resident here were included. The sample was massive, comprising 1,461,874 men and 842,799 women, each assigned to one of 223 occupational groups. Occupation and date of birth has never before been studied so systematically.

Smithers's study involved an astrological analysis and a season of birth analysis. Our critique and re-analysis are concerned mostly with the former, but the latter will be included where relevant.

### Smithers's Astrological Analysis and Results

Smithers's approach was simply to compare the observed sun-sign distributions with the results predicted in advance by 15 of the "most expert and well-qualified astrologers in the country" and by Charles Harvey, president of the Astrological Association. The 15 astrologers were asked to indicate which three occupations were most strongly associated with each sun-sign and which two sun-signs were most strongly associated with the 223 occupations in the study. Charles Harvey made several predictions, e.g., that nurses, trade-union officials, and salesmen would be more often born under alternate sun-signs.

Smithers gives the sun-sign occupations most frequently chosen by his 15 astrologers and for which census data was available. The result was a total of 36 occupations (see Table 1). For each occupation Smithers also gives a graph of the actual sun-sign distribution relative to the general population "so you can see for yourselves how well you think they did."

Smithers concluded that the astrologers did fairly well and that Harvey's predictions for nurses and trade-union officials were strikingly confirmed. Smithers stated: "The study does not offer dramatic support for astrology. But there are a sufficient number of odd instances to give pause. There were, for example, Charles Harvey's predictions for nurses and trade union officials, and the occasions when the panel of astrologers were correct. . . ." He then qualified this with: "Nevertheless, I remain unconvinced. Most of the hits can be explained in other ways." This statement was followed (1984d) by a discussion of these other ways—namely, seasonal factors, social habits, and belief in astrology.

Smithers's study was followed in the same newspaper by a commentary from Charles Harvey (1984). Harvey pointed out that (1) astrologers would not consider sun sign to be a dominant factor in revealing job preference, and (2) signs are traditionally related to personality and individual differences that can vary widely within a single occupation—for example, cooks may be Taurus, Cancer, or Sagittarius, depending on whether they are inclined toward haute cuisine, health food, or fast food, respectively. Nevertheless, despite these limitations and Smithers's cautious

TABLE I

Occupations Likely to Be Associated with Each Sun Sign

| Sun Signs | Occupations |
|---|---|
| Aries | Athlete, dentist, car mechanic, fireman. |
| Taurus | Accountant, architect, farmer, baker. |
| Gemini | Author/journalist, commercial traveler, teacher. |
| Cancer | Cook, social worker |
| Leo | Actor/musician, manager. |
| Virgo | Clerk/cashier, optician, typist. |
| Libra | Hairdresser/beautician, tailor/dressmaker. |
| Scorpio | Doctor, miner, policeman. |
| Sagittarius | Athlete, clergyman, lawyer. |
| Capricorn | Civil servant, civil engineer, politician, miner. |
| Aquarius | Electrician, radiographer. |
| Pisces | Actor/musician, artist, barman/maid, fisherman. |

The occupations are those most frequently chosen by Smithers's panel of 15 expert astrologers and for which census data was available. In the census data the sample size for each occupation ranges from 613 (opticians) to 233,516 (clerks and cashiers), median 5,000. When the three highest and three lowest sample sizes are excluded to avoid bias from extreme values, the range for the remaining 30 occupations is 1,177 to 28,491, average 9,712.

*Source:* Smithers 1984a.

assessment, Harvey was able to conclude that Smithers's study was one of an unspecified number of recent studies that promised the return of astrology as a serious science. He pointed out that astrology "does allow us to make . . . predictions for subsequent testing. The 'seasonal' explanation of Professor Smithers does not allow us to do this—a simple example of the relative sterility of such a theory despite its possible value in other respects."

In a lively response from *Guardian* readers (March 24, 27, and 31, 1984), scientists disagreed with both Harvey and Smithers, pointing out that random fluctuations and social factors were sufficient to explain the results.

We agree with this assessment. In what follows we show that non-astrological factors can explain all the results.

## Our Re-analysis

Smithers counted the predicted sun-sign as a hit if its observed frequency was above average. He gives the results for ten occupations on which the astrologers were in good agreement (see Table 2). These results show that 8 out of 11 predictions were hits. How does this compare with the number expected by chance?

TABLE 2

Sun Sign vs. Census Data

| Occupation | Social Class | Predicted Sign | Most Frequent Sign |
|---|---|---|---|
| Coal Miner | 4 | *Scorpio, Capricorn* (HIT) | Sagittarius |
| Car mechanic | 3 | *Aries* (HIT) | Pisces |
| Tailor/dressmaker | 3 | *Libra* (HIT) | Capricorn |
| Baker | 3 | Taurus (HIT) | Virgo |
| Clerk/cashier | 3 | Virgo (HIT) | Libra |
| Typist (=secretary) | 3 | Virgo | Cancer |
| Hairdresser | 3 | Libra | Leo |
| Optician | 1 | *Virgo* (HIT) | Gemini |
| Civil Engineer | 1 | Capricorn | Cancer |
| Author/journalist | 2 | *Gemini* (HIT) | *Gemini* (HIT) |

If each sign has an equal chance of being above average, then the expected number of hits will be half the number of predictions. But, as shown in Figure 1, there are marked seasonal differences between social classes, with Virgo through Pisces being below average for the upper classes and above average for the lower classes. This means that if the occurrence of signs and classes is unbalanced—as it is in Table 2, where Virgo through Pisces and the lower classes are both overrepresented—then each sign will not have an equal chance of being above average, and the expected number of hits will not be half the number of predictions. In fact, in Table 2 the number of hits expected on seasonal grounds is 7, hence 8 hits by astrology is unremarkable. In other words, as Smithers himself

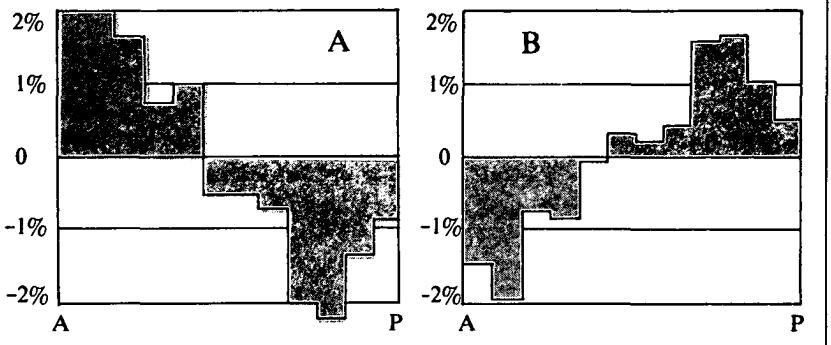


FIGURE 1. Distribution of sun signs in Britain according to social class.

Data are for a 10 percent sample of the UK 1971 census.

In each graph the horizontal axis from left to right indicates the sun sign in sequence from Aries through Pisces.

A = social classes 1 (professional, e.g., doctors, scientists, clergy) and 2 (intermediate, e.g. managers, teachers, nurses). total sample 463,702 or 21 percent of the total.

B = social classes 3N (skilled nonmanual, e.g., clerks, cashiers, typists), 3M (skilled manual, e.g., carpenters, electricians, hairdressers), 4 (partly skilled, e.g., storemen, packers, hotel workers), and 5 (unskilled, e.g., laborers, office cleaners). Total sample 1,765,612 or 79 percent of the total.

Sixty-three percent of the total sample were male. There was no appreciable difference between the sun-sign distributions for males and those for females.

The difference between the two graphs is striking and is readily explained by the differences in socioeconomic factors. For example, social classes 1 and 2 would be more likely to plan spring births to avoid the winter, especially as most of the sample were born before the advent of central heating. The difference is not explained by sampling variations (see text), which for these large sample sizes are very small, namely 0.49 percent for A and 0.25 percent for B. This finding is consistent with results obtained in other studies on seasonal differences in birth rates (for a review, see Kevan 1979).

*Source:* Smithers 1984d.

points out (1984d), most of the predictions "depended on the seasonal patterns for their successes."

Smithers does not apply the more rigorous test of comparing predictions with the most frequent sign. Table 2 shows that in this case the number of hits is one, which is exactly the number expected by chance.

Smithers excludes occupations for which his 15 astrologers were in less agreement. To find out what happens when no exclusions are made and other expert astrological opinions are included, we compared Smithers's census data with the sun-sign occupations given by (1) Table 1, (2) D. Parker and J. Parker (1971), which is representative of serious astrology books in the United Kingdom, and (3) C. J. Puotinen's (1980) survey of United States sources. In each case the analysis was straightforward, because neither Virgo through Pisces nor the lower social-classes were overrepresented. The results, summarized in Table 3, show that only Smithers's astrologers achieved a significant level of hits.[1] However, this is largely due to the results given in Table 2, and when these are excluded the results become nonsignificant.

TABLE 3

Astrologers' Predictions vs. Census Data

| Source | No. of Predictions | Predicted sign is above average | | Predicted sign is the most frequent | |
|---|---|---|---|---|---|
| | | Observed | Expected | Observed | Expected |
| Smithers's panel | 37 | 25 | 18.5 | 5 | 3.1 |
| Parker and Parker | 59 | 32 | 29.5 | 4 | 4.9 |
| Puotinen | 106 | 61 | 53.0 | 10 | 8.8 |

The table shows the observed number of hits and the number expected by chance. By chi-squared test with Yates's correction none of the results are significant below the $p = .15$ level except the 25 hits by Smithers's panel, for which $p = .05$.

Although generally nonsignificant, the results shown in Table 3 are nevertheless generally in the direction predicted by astrology. Does this provide some support for sun-sign astrology? The answer is no, because there is a simple, nonastrological explanation. H. J. Eysenck and D. K. B. Nias (1982) have shown that people who know the meaning of their sun-sign tend to display self-attribution—that is, they measurably shift their self-image and behavior toward the descriptions given by astrology, whereas those who are ignorant of the meaning display no measurable effect.[2]

Because most of the occupational correspondences tested by Smithers have been widely disseminated as part of sun-sign lore, it is reasonable to ascribe the results of Table 3 to self-attribution.[3]

Smithers suggests that self-attribution is unlikely to affect job choice. He acknowledges that self-attribution is a factor in personality, but states that "the serious business of getting a job though is something else" (1984a). However, few people have much help in choosing their job. Instead, their choice is influenced by their interests and abilities as assessed by themselves, their parents, and their prospective employers, which is precisely the situation that could be influenced by knowledge of sun sign.[4] Furthermore job choice is far from being inexorably destined—even those who rise to eminence in a given profession have frequently chosen it by chance or accident. H. J. Eysenck is one example (he originally wanted to study physics); see A. Bandura (1982) for others. So, if chance or accident can play a role, why not self-attribution?

Given that self-attribution can conceivably affect job choice, the important question that Smithers fails to address is: How much self-attribution is required to explain the results? To answer this we first have to establish how much result there is to explain. We cannot ascribe all the ups and downs in Smithers's graphs to astrology because this would ignore the random variations inherent in the sampling procedure. (It is because of sampling variations that the hands dealt in a card game seldom contain equal numbers of suits.) So we measured the overall variation in nine of Smithers's graphs for sample sizes between 1,800 and 22,000 (which covers 70% of his graphs) and subtracted the theoretical sampling variation. The result was the variation to be explained by factors other than sampling variations; it averaged a little less than 2 percent.[5]

We cannot ascribe all of this variation to astrology, because other factors, such as the availability of jobs, play a part. But at least it establishes an upper bound. So we can say that Smithers's results can be explained if, at most, about 1 person in 60 is influenced by astrology in their choice of job. How does this compare with the level of self-attribution in the general population?

Some clues are provided by G. A. Dean (1983), who briefly surveyed the dozen available studies of self-attribution and sun sign, as well as various national opinion polls of belief in astrology. The indications were consistent and suggested that roughly 1 person in 3 not only believes in astrology but believes in it sufficiently to measurably shift their self-image in the corresponding direction.[6] Therefore, to explain our result it requires about 1 in 20 of those who have shifted their self-image to have shifted their job preference as well. This does not seem improbable.

## Do Smithers's Results Replicate?

Smithers's analysis rests on the comparison between astrologers' predictions

and the peaks in his graphs, but he presents no statistical analyses. In their letters to the *Guardian* the scientists ask the obvious questions. How reliable are the peaks? Do they replicate? And are they statistically significant? The point here is that unless the results replicate we cannot be sure that anything special is happening. In his reply (1984e) Smithers says: "Statistical analysis, omitted on the advice of the *Guardian,* which thought its readers would not be interested in the details, suggests that there is indeed something in the data. Also, where it has been possible to divide the sample . . . similar trends have been found in the two parts; the patterns for men and women in the same occupation can be very much alike and, where occupations are aggregated into social classes, the clear separation with time of year occurs."

Unfortunately this reply tells us little about whether the sun-sign results replicate. However, during a subsequent discussion of Smithers's study at the 4th International Astrological Research Conference in London on October 27-28, 1984, Smithers stated that he had not divided his sun-sign samples in half to see if the results replicated. This alone is enough justification to dismiss his study out of hand.[7]

## Sun-Sign Alternation

Smithers makes much of Harvey's successful prediction that people in the caring professions (specifically nurses, sample size 35,039) should be born more frequently in feminine (even-numbered) signs, beginning with Taurus, and that trade-union officials (sample size 742) should be born more frequently in assertive or masculine (odd-numbered) signs, beginning with Aries. However, of the occupations listed in Table 1, nurses are the most caring and *feminine* of all (with perhaps hairdressers in second place) and trade-union officials are the most assertive and *masculine* of all (with perhaps policemen and lawyers in second place). In other words, these occupations are the ones most clearly related to sign alternation and therefore are the ones that should most clearly display the corresponding effects of self-attribution. So we cannot conclude that the results are necessarily evidence for genuine astrological effects.

There is another problem here that Smithers fails to address. A large number of occupations are involved. Could the results be due to chance? This is especially pertinent for the the trade-union officials because their sample size is the second lowest of all the occupations studied; hence sampling variations probably swamped everything else and rendered the results meaningless (a point ignored by Smithers).

To find out, we counted for each occupation the number of peaks in odd-numbered signs and the number of peaks in even-numbered signs, and took whichever number was higher. We defined a peak as a sign total that had lower totals on either side regardless of whether it was above or below average; in this way we addressed alternation directly. We also

counted for each occupation the number of peaks according to element (fire, earth, air, or water) and again took the highest number.

The results are given in Table 4. They indicate that in Smithers's graphs overall there is nothing in the alternation of ups and downs that cannot be explained by chance.

TABLE 4

Distribution of Sun-Sign Peaks in Census Data

| Test | No. of peaks | Frequency | | Significance by chi-squared test |
|---|---|---|---|---|
| | | Observed | Expected | |
| Odd or even | 1 | 1 | 0.3 | $X^2 = .44$ |
| | 2 | 14 | 12.9 | $df = 3$ |
| | 3 | 15 | 15.5 | $p = .9$ |
| | 4 | 6 | 7.0 | |
| | 5 | 2 | 2.0 | |
| | 6 | 0 | 0.3 | |
| Element | 1 | 5 | 6.0 | $X^2 = .26$ |
| | 2 | 27 | 26.7 | $df = 2$ |
| | 3 | 6 | 5.3 | $p = .9$ |

Table 4 shows the results of tests applied to the 38 sun-sign distributions given by Smithers. The corresponding occupations are those given in Table 1 plus nurses and trade-union officials. The expected frequencies are those expected by chance and were determined by computer simulation using one million replications for the odd-even test and a quarter of a million replications for the element test. If a correlation with alternate signs or element exists as predicted by sun-sign astrology, the distribution of peaks should be skewed toward the higher numbers. The element results show such a skew but it is very small; the odd-even results show a slightly larger skew but in the wrong direction; overall, the results offer no support for sun-sign astrology. In both tests the difference between observed and expected frequencies is decidedly nonsignificant.

*Source:* Smithers 1984a; 1984b.

Some of the elements with at least two peaks (three is the maximum possible) were those that might have been predicted by astrology; for example, commercial travelers were "fire," policemen were "earth," and authors were "air." But many more were not. For example, farmers and

coal miners were "fire" not "earth," firemen were "earth" not "fire," fishermen were "earth" not "water," politicians were "water" not "fire" or "air," and car mechanics were "water" not "earth."

Do such discrepancies and the chance results of Table 4 conflict with our explanation based on self-attribution? The answer is no, because elements are seldom part of sun-sign lore at its most popular level—at least as popularly remembered. (Do you know which element your sign is?) For example, Pisces is associated with fishermen not because it is a water sign but because it involves fish. In fact, the results strengthen the case for self-attribution and weaken it for astrology, because popular lore shows a slight bias in favor of astrology (Table 2), whereas the more subtle underlying astrological lore does not (Table 4). This is precisely the result to be expected if sun-sign astrology was nothing more than self-attribution.[8]

## Conclusion

Professor Smithers has produced an interesting study that allows the most detailed look yet at the relationship between sun sign and occupation. We agree with him that there is "something in the data," but we disagree that it reflects a genuine astrological effect. Our re-analysis of Smithers's results shows that they can be explained by statistical fluctuations and self-attribution effects. As pointed out by Charles Harvey, the study has limitations as a test of sun-sign astrology; nevertheless we suggest that if there were underlying truths they would shine through despite these limitations. They certainly do not support sun-sign astrology.

## Notes

1. Owing to space constraints we are unable to include the actual comparison. Interested readers can obtain details from Prof. I. W. Kelly, Department of Educational Psychology, University of Saskatchewan, Saskatoon, Canada, S7N 0W0.

2. The same thing applies to our perception of others. Having been told what a person is like, we often adjust our perceptions to suit, regardless of whether the information is correct. Such effects are more pervasive than commonly assumed (Cooper and Good 1983).

3. Empirical sun-sign studies of occupation seldom replicate even when the sample size is several thousands, see Dean and Mather (1977), pages 114-117. Furthermore, as Smithers points out, his expert astrologers do not use sun signs in the way implied here. Hence the astrologers will have based their opinions not on empirical studies but ultimately on what they read in astrology books. Because both popular and serious astrology books merely repeat the same traditional ideas, everyone—astrologers and lay people alike—is exposed to the same indoctrination. This provides ideal conditions for self-attribution effects and other self-fulfilling prophecies.

4. And of course by astrological business counseling. Today, in the United

States at least, choosing employees by astrology is practiced at all levels of astrological sophistication. In the San Francisco Bay area, students at the University of California at Berkeley complain that local employers use sun signs to discriminate among job applicants. "It's just too bad if you're Scorpio or Taurus—you won't get the job (Bastedo 1978). The extent to which this would apply to Smithers's census data is unknown but probably very small. Nevertheless it is obviously a factor that has to be kept in mind, especially as even small effects will manifest if the sample is large enough.

5. This variation is a standard deviation expressed as a percentage of the mean frequency per sun sign.

6. Supporting evidence is provided by G. Jahoda (1954). The Ashanti people in Ghana automatically include in their individual names the week in which they were born. From juvenile court records Jahoda found that Ashanti boys whose name meant "quiet and peaceful" were half as numerous among 416 aggressive delinquents as other boys, whereas those whose name meant "quick-tempered and aggressive" were twice as numerous. This is consistent with about half the population being affected by self-attribution to a measurable extent.

7. Smithers (1984c) gives data for only one occupation that allows the reproducibility to be checked—namely, 46,756 teachers in primary and secondary schools, 14,867 teachers of further education, and 2,304 teachers in universities. The broad trends in each case are similar and consistent with the effect of social class; for example, Aries is above average and Sagittarius is below average in all three. But there are important differences in detail; for example, Libra is well above average for the first and well below average for the others, and Cancer is the least frequent sign for the first and above average for the others. Because the sample sizes for the first two are well above average (which means that the effect of sampling variations is relatively small), the chances do not seem good for impressive replication of individual signs among the samples as a whole, which is bad news for sun-sign astrology.

8. It is worth noting that, when social class is allowed for, the odd-even sign alternation is about all that is left for self-attribution and astrology to explain. Hence alternation tests are especially crucial.

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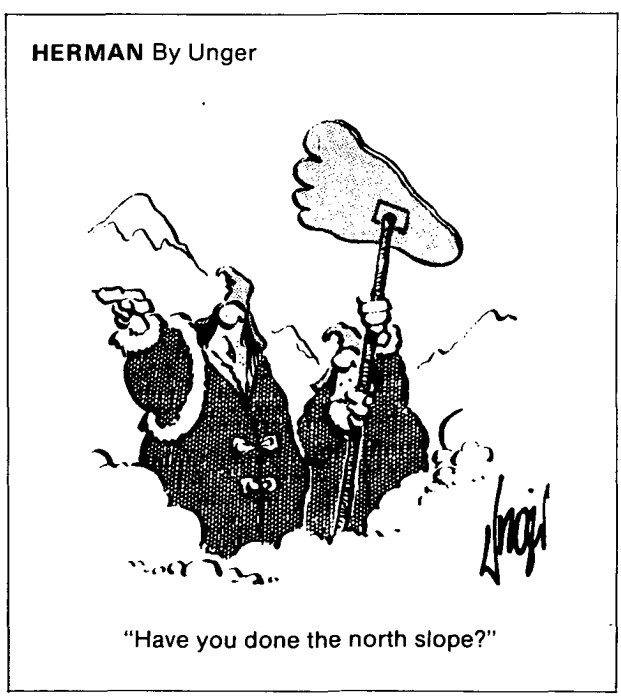


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# Astrological Forecasts and the Commodity Market: Random Walks As a Source of Illusory Correlation

*Investors who look to the stars for guidance can easily be fooled by drifting series called "random walks."*

James Rotton

IF DANIEL PALLANT is right and I'm wrong, I will have missed out on the chance of a lifetime to nearly double my money in less than three months. Daniel Pallant is the director of Commodity Consultants, Ltd., of Surrey, England. On more than one occasion, he has suggested that investors should look to the stars before they trade gold, copper, and other commodities.

According to Pallant, "all commodity and financial markets in general respond to astrological events, and therefore anyone trading and hoping to make money must study these effects or be advised by someone who makes it his business to do so." This claim appeared on page 10 of the October 21, 1983, issue of *Financial Weekly*. Pallant has issued similar claims in *Metal Bulletin* (September 16, 1983), the *Krugerrand Bulletin* (December 15, 1983), and *Futures World* (March 1984). In *Futures World,* for example, he claimed that gold prices are "affected when the sun changes sign, that is, when it crosses the boundary between one sign and the next" (p. 18).

However, Pallant cautions, "there are many other planetary ingress and aspect cycles operating at the same time" (p. 18). What are they? After alluding to sun/Jupiter, sun/Saturn, sun/Venus, and other relationships, he tells us that "the concepts are too complex to be discussed here." Since Pallant is a director of a consulting firm, it is not surprising that he's reluctant to disclose his methods: "It is not, of course, possible in an article to explain the whole process, nor would we be willing to divulge the

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results of expensive research and its valuable product for commercial purposes."

It occurred to me that metal prices might be attuned to planetary positions if a small but influential number of investors visit astrologers before they purchase commodities. Astrological forecasts might lead to actions that confirm people's beliefs. This is of course what social scientists call a "self-fulfilling prophecy." It is what analysts (e.g., B. G. Malkiel [1973]) call "the castle in the air theory" of investing. If enough investors believe that a stock is on its way up—that is, if they build their hopes into castles in the air—each will be willing to pay more for the stock, sending its price up.

A good example of how the self-fulfilling prophecy works is provided by investors who listened to Joseph Granville, a consultant who operates out of Florida. On January 6, 1982, Granville wired clients: "Stock market will nosedive. Sell tomorrow." The next day, on January 7, the Dow-Jones average dropped 23 points, and more than $40 billion was lost in trading (Myers 1983). In this case, more than one investor ended up in the castle's cellar.

Thinking that Pallant might be taking advantage of people's willingness to build castles in the air, I wrote him on April 16, 1984. In my letter, I asked if he thought that some of the trends he had observed might "stem from the fact that some investors may base their decisions on what they believe the stars portend." Pallant responded to my letter with one dated April 22, which said in part:

> I have discovered certain correlations which are not obvious to an even experienced astrologer, and would not be taken into consideration by an ordinary investor. These discoveries have required patient computer work over a considerable length of time, and many of the conclusions derived from it negate the fundamentals of traditional astrology.

In more than one article, Pallant has dismissed traditional approaches to astrology, and it might be wondered if he is advancing a new astrological theory. Therefore, in my letter, I asked Pallant if he would be willing to speculate about the "remarkable correlations" he had observed. He was, and I believe his response is worth quoting at some length:

> The most likely explanation for these effects lies in resonance theory. Tiny forces rhythmically over many thousands of years have sensitized the human race to their effects. Although it appears that, as individuals, we have only just arrived and are discrete entities, in truth, the chemical code which determines our nature at birth has lain dormant in our forebears for many hundreds of centuries. . . . We have, in effect, experienced the cycles of the planets and the other bodies of the Universe for this entire period, and are in consequence very sensitive to them.
>
> If it is possible to show that economic cycles correlate with planetary

and other cosmic cycles, which I contend it is, then the study of these effects is of paramount importance to the human race.

Taken as a whole, Pallant's letter leads me to conclude that he is probably misguided rather than mendacious. From those who have had dealings with him, I have learned that he sincerely believes that metal prices are determined by the stars. But the fact remains that Pallant earns a commission when clients heed his advice; and, if he really believes that the stars provide a clue to the commodities market, I have to wonder why he is telling others how to get rich. Has he gambled any of his own money on his predictions? And, if he has, what has been the outcome?

Rather than back up his claims by disclosing how much money he himself has made, Pallant presents charts that seem to suggest he has been successful in predicting gold and copper prices. All of his charts describe past successes—postdiction rather than prediction. They resemble the one in Figure 1, which I prepared in order to check out one of Pallant's few predictions. In the December 15, 1983, issue of the *Krugerrand Bulletin,* Pallant predicted (p. 9): "The most probable time for another useful rally in the price [of gold] will be after January 20th, lasting until the first week of February. Thereafter, until the end of June, the gold price will continue to be under pressure in a period punctuated by occasional rallies as it builds between $390 and $440 per ounce."

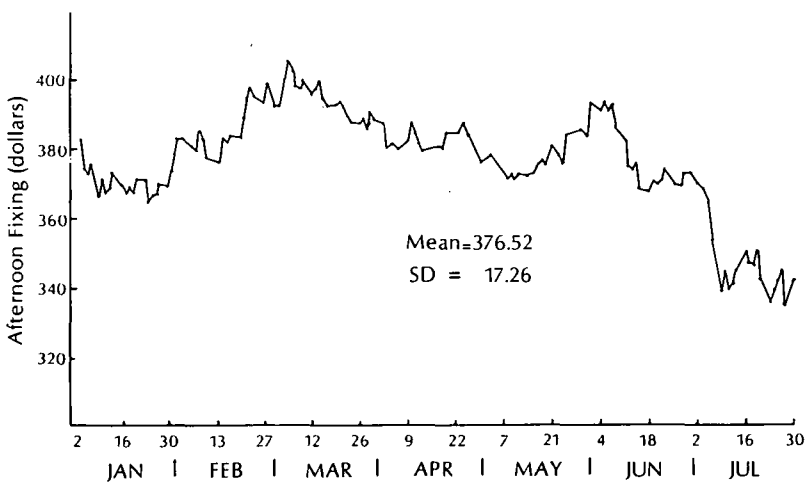


FIGURE 1. Afternoon gold fixings on the London market between January 2 and July 30, 1984.

*I anticipate that the most probable time for another useful rally in the price will be after January 20, lasting until the first week of February. Thereafter, until the end of June, the gold price will . . . build a base between $390 and $440 per ounce. . . . I think that it is unlikely that the gold price will rise dramatically until the third quarter of 1984, when it could well reach $600 per ounce.*

—Daniel Pallant, Market Astrologer

The chart in Figure 1 gives afternoon fixings for gold, in London, from January 2 to July 30, 1984. For this chart, we can see that gold closed at $371.25 on January 20. Prices rose slightly until February 3, but the rise was not nearly so great as Pallant claimed in a subsequent article. In the March 1984 *Futures World,* he stated that gold prices "rose $25 reaching its high on Friday Feb. 3" (p. 19). Maybe there was a time, on February 3, when gold prices reached a high of $396.25 (i.e., 371.25 + 25.00), but the afternoon fixing for that date was $383.10, and the difference between prices on February 3 and January 20 was only $11.25.

Taking a closer look at the chart in Figure 1, we can see that gold prices at the end of June are nowhere near the $390 that Pallant predicted. They also appear to be declining. In the *Krugerrand Bulletin,* Pallant went on to predict that "it is unlikely that the gold price will rise dramatically until the third quarter of 1984, when it could well reach $600 per ounce." On July 30, 1984, as I write this article, gold closed at $342.35.*

Gold prices are an example of what mathematicians (e.g., W. Feller [1968]) call a "random walk." In a random walk, values drift up and down in an unpredictable fashion. You can produce a good facsimile of the chart in Figure 1 by flipping a coin. Simply give yourself one cent when the coin comes up heads, and take away a penny when it comes up tails. It may be true that you have a 50-50 chance of heads on each trial, and you would be surprised if the coin came up heads on the first eight flips. However, the probability of eight (and even nine or ten) heads in a row is

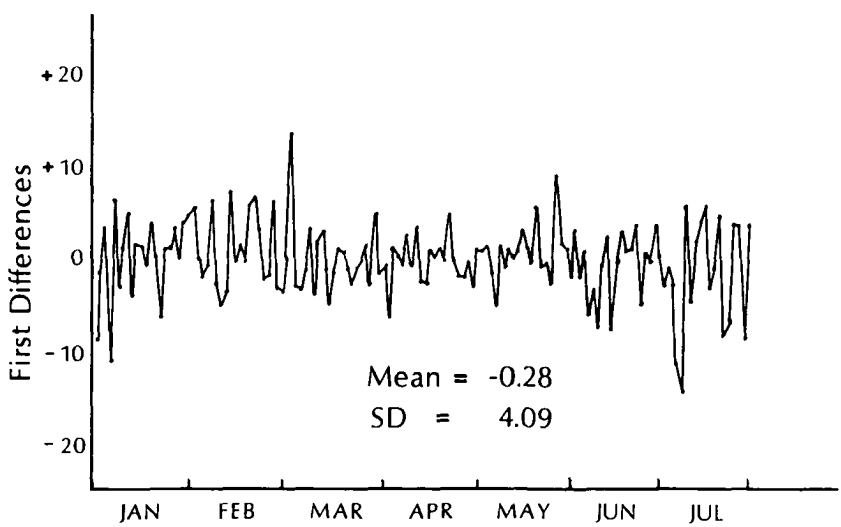


FIGURE 2. Daily differences in afternoon gold fixings on London exchange between January 2 and July 30, 1984. (Note: Weekends and holidays, when market was closed, are not listed.)

* Gold was welling at $344.15 an ounce when the London market closed on Friday, September 29, 1984.—*Ed.*

greater than zero, and sooner or later you'll find that your earnings appear to be drifting upward (if you're lucky) or downward (if you're not).

Random walks lead to "gambler's ruin." As Weaver (1963) noted in a delightful little book called *Lady Luck,* the best way to lose your shirt is to think that you have discovered a pattern in a game of chance. Playing roulette, for example, I would be surprised if a ball landed on Red twenty times in a row, and I might even jump to the conclusion that the wheel was fixed. But if the game is an honest one, the odds of Red coming up again is still 50-50.

How can investors tell when they are being taken on a random walk? Very simply: All they have to do is subtract each day's price from the price on the preceding day; that is, for the mathematically minded, $Z_t = X_t - X_{t-1}$, where $X_t$ is any day's closing. This is a reasonable operation, since most investors are much more interested in a stock's ups and downs (earnings and losses) than they are in absolute prices. As Figure 2 suggests, day-to-day changes in gold prices are about as random as points earned by flipping a coin. For those who correctly distrust interocular ("eyeball") tests, I should point out that this conclusion is supported by statistical tests for randomness.*

The thing that makes random walks pernicious—a real pain for investors—is that they give rise to correlations within a series. Unfortunately, the higher the correlation within a series, the less faith we can put in correlations between series. Referring back to Figure 1, we can see that gold prices declined from $383.00 on January 2 to $342.25 at the end of July. But temperatures outside the London commodity exchange increased during the same period. Shall we conclude that high temperatures lead to lower prices?

We can draw absolutely no conclusions about relationships between series (e.g., the stars and the gold market) when one of them is drifting. This point can be illustrated by considering another economic series: the price of cotton. In an article social psychologists used to cite, C. I. Hovland and R. R. Sears (1940) reported what appeared to be a reliable correlation between cotton prices and the number of lynchings in the south. From 1882 to 1930, lynchings increased as prices declined. Although this finding was seized upon as evidence for the frustration-aggression hypothesis, A. Mintz (1946) showed that it stemmed from the fact that previous cotton prices had been increasing while the number of lynchings had been declining. For the same period of time, there was also positive correlation between the number of telephones and suicides in the United States! As

* The difference scores in Figure 2 pass two tests suggested by G. E. P. Box and C. C. Jenkins (1976) for identifying a white noise (or random) series of observations. First, nonsignificant autocorrelations of − .07 and − .06 were obtained when differenced scores were lagged one and two days, respectively. Second, for 25 lags, this series' Ljung-Box Q-value was 21 $p < .68$. For more on time-series analysis, the reader is referred to books by J. M. Gottman (1982) and R. McCleary and R. A. Hay, Jr. (1980).

E. T. Webb and his colleagues (1981) have pointed out, all of these correlations stem from trends in individual series.

Random walks give rise to "illusory correlations," which lead one to conclude that events are related when they are not. It is my guess that this cognitive bias is more pronounced among "chartists," who spend their days pouring over graphs, than among other types of investors and financial advisors. For example, as part of a class exercise, B. G. Malkiel (1973, 120) had students generate charts by flipping coins: "One of the charts showed a beautiful upward breakout from an inverted head and shoulders (a very bullish formation). I showed it to a chartist friend of mine who practically jumped out of his skin. 'What is this company?' he exclaimed. 'We've got to buy immediately. This pattern's a classic. There's no question the stock will be up 15 points next week.' He did not respond kindly to me when I told him that the chart had been produced by flipping a coin."

However, although many series drift in an apparently random fashion, it should not be concluded that investors are behaving in a totally capricious manner. Rather, each investor tries to maximize individual gains and minimize losses—an economic definition of rationality—but, because future events are uncertain, their individualistic actions combine in a stochastic fashion to generate a random walk. I would imagine that there are also a number of natural and reasonable correlations with gold prices; for example, it makes sense to assume that gold prices go down as stock prices go up. On the other hand, prices go up when people hoard gold as an edge against inflation, especially during times of war. But war is usually an unexpected event; and, even if the price of gold goes up when the stocks go down, one is left with the difficult job of predicting when stocks will decline in value. As more than one analyst (e.g., E. E. Fama [1965]) has shown, stocks also drift up and down in a random fashion.

Clearly, some knowledge of random-walk theory and, more generally, time-series analysis is essential for those of us who wish to challenge astrological claims. In some cases, such claims can be easily dismissed by showing that apparent relationships disappear when a time-series is differenced—that is, by looking at ups and downs rather than absolute values. In other cases, spectral analysis and more sophisticated procedures may be required (see Kelly and Rotton 1983; Rotton and Kelly 1985; Rotton, Kelly, and Frey 1983). In one study (Rotton and Rosenberg 1984), a student of mine uncovered what appeared to be a reliable relationship between phases of the moon and Dow-Jones closing averages. However, the relationship vanished when closings were transformed into an uncorrelated (i.e., serially independent) set of observations.

Our findings might not surprise an econometrician, but drift makes things difficult for those of us who wish to counter astrological claims. The time has passed when astrologers based their claims upon one or two spectacular predictions. Those who claim that the stars play a role in human affairs can now take advantage of data networks like Dow-Jones

News/Retrieval (R) and computers that can churn out thousands of correlations. As J. Agassi (1979, 112) has observed, "since astrologers are now using computers and given enough trials, then by Bernoulli! every correlation may be hit upon." Further, because stocks and other commodities drift up and down, astrologers will be able to point to spuriously significant correlations that appear to support their beliefs and prejudices.

All of us fall victim to illusory correlations at one time or another, especially when we want to believe that two events are related (Kahneman and Tversky 1972). Unfortunately, the more we want to believe that events are related, the more likely we are to be fooled into mistaking chance for correlation (Myers 1983). But Pallant is not only capitalizing on illusory correlations and people's desire for riches; he is also capitalizing on the fact that one can always uncover apparently significant correlations when a series drifts over time.

Thus, even if I'm right and gold prices don't reach $600 by the third quarter of this year, Pallant will probably be able to point to a prediction that made money for his clients. While gold prices have drifted downward, other commodites have drifted upward. Some of Pallant's clients may even realize profitable returns on their investments. But has Pallant's overall performance exceeded chance? Have his clients done as well as they would if they had based their decision to buy and sell on the flip of a coin? Have they made more than somebody who bought a random portfolio of stocks at the beginning of the year and held onto them? And, finally, I keep wondering, how much gold did Pallant buy in December 1983, when it was selling at $380 an ounce and he made his prediction?

## Acknowledgment

I would like to thank I. W. Kelly for calling my attention to Pallant's predictions and commenting on an earlier draft of this paper.

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# The Hundredth Monkey Phenomenon

*A remarkable New Age claim of 'collective consciousness'—a 'spontaneous' appearance of potato-washing behavior in a group of monkeys—falls to scrutiny. The way belief in this claim grew exemplifies the methodology of pseudoscience.*

Ron Amundson

CLAIMS OF THE paranormal are supported in many ways. Personal reports ("I was kidnapped by extraterrestrials"), appeals to puzzling everyday experiences ("Did you ever get a phone call from someone you had just dreamed about?"), and references to "ancient wisdom" are a few. Citations of actual scientific results are usually limited to ESP experiments and a few attempts to mystify further the already bizarre discoveries of modern physics. But the New Age is upon us (we're told) and New Age authors like Rupert Sheldrake (1981) and Lyall Watson (1979) support their new visions of reality with scientific documentation. Sheldrake has a bibliography of about 200 listings, and Watson lists exactly 600 sources. The sources cited are mostly respectable academic and scientific publications. The days of "[unnamed] scientists say" and "Fred Jones, while walking alone in the woods one day . . ." are gone. Or are they?

I teach college courses in epistemology, in the philosophy of science, and in pseudoscience and the occult. Students in these courses naturally bring to class examples of remarkable and paranormal claims. During the past few years one such claim has become especially popular, the "Hundredth Monkey Phenomenon." This phenomenon was baptized by Lyall Watson, who documents the case with references to five highly respectable articles by Japanese primatologists (Imanishi 1963; Kawai 1963 and 1965; Kawamura 1963; and Tsumori 1967). Watson's discussion of this phenomenon covers less than two pages. (Except where noted, all references to Watson are to pages 147 and 148.) But this brief report has inspired much attention. Following Watson, a book (Keyes 1982), a newsletter

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From Masao Kawai, *Primates*, 6 1-30, 1965

One of the monkeys washing a sweet potato

article (*Brain/Mind Bulletin* 1982), and a film (Hartley 1983) have each been created with the title "The Hundredth Monkey." In addition we find a journal article entitled "The 'Hundredth Monkey' and Humanity's Quest for Survival" (Stein 1983) and an article called "The Quantum Monkey" in a popular magazine (*Science Digest* 1981). Each relies on Watson as the sole source of information on the remarkable and supernatural behavior of primates.

The monkeys referred to are indeed remarkable. They are Japanese macaques (*Macaca fuscata*), which live in wild troops on several islands in Japan. They have been under observation for years. During 1952 and 1953 the primatologists began "provisioning" the troops—providing them with such foods as sweet potatoes and wheat. This kept the monkeys from raiding farms and also made them easier to observe. The food was left in open areas, often on beaches. As a result of this new economy, the monkeys developed several innovative forms of behavior. One of these was invented in 1953 by an 18-month-old female that the observers named "Imo." Imo was a member of the troop on Koshima island. She discovered that sand and grit could be removed from the sweet potatoes by washing them in a stream or in the ocean. Imo's playmates and her mother learned this trick from Imo, and it soon spread to other members of the troop. Unlike most food customs, this innovation was learned by older monkeys from younger ones. In most other matters the children learn from their parents. The potato-washing habit spread gradually, according to Watson, up until

1958. But in the fall of 1958 a remarkable event occurred on Koshima. This event formed the basis of the "Hundredth Monkey Phenomenon."

### The Miracle on Koshima

According to Watson, all of the juveniles on Koshima were washing their potatoes by early 1958, but the only adult washers were those who had learned from the children. In the fall of that year something astounding happened. The exact nature of the event is unclear. Watson says:

> . . . One has to gather the rest of the story from personal anecdotes and bits of folklore among primate researchers, because most of them are still not quite sure what happened. And those who do suspect the truth are reluctant to publish it for fear of ridicule. So I am forced to improvise the details, but as near as I can tell, this is what seems to have happened. In the autumn of that year an unspecified number of monkeys on Koshima were washing sweet potatoes in the sea. . . . Let us say, for argument's sake, that the number was ninety-nine and that at eleven o'clock on a Tuesday morning, one further convert was added to the fold in the usual way. But the addition of the hundredth monkey apparently carried the number across some sort of threshold, pushing it through a kind of critical mass, because by that evening almost everyone was doing it. Not only that, but the habit seems to have jumped natural barriers and to have appeared spontaneously, like glycerine crystals in sealed laboratory jars, in colonies on other islands and on the mainland in a troop at Takasakiyama.

A sort of group consciousness had developed among the monkeys, Watson tells us. It had developed suddenly, as a result of one last monkey's learning potato washing by conventional means. The sudden learning of the rest of the Koshima troop was not attributable to the normal one-monkey-at-a-time methods of previous years. The new phenomenon of group consciousness was responsible not only for the sudden learning on Koshima but for the equally sudden acquisition of the habit by monkeys across the sea. Watson admits that he was forced to "improvise" some of the details—the time of the day, the day of the week, and the exact number of monkeys required for the "critical mass" were not specified in the scientific literature. But by evening (or at least in a very short period of time) almost everyone (or at least a large number of the remaining monkeys) in the colony had suddenly acquired the custom. This is remarkable in part because of the slow and gradual mode of acquisition that had typified the first five years after Imo's innovation. Even more remarkable was the sudden jumping of natural boundaries, apparently caused by the Koshima miracle.

### Documentation

In this section I investigate the relations between Watson's description of

the Hundredth Monkey Phenomenon and the scientific sources by which he validates it. To be sure, we must not expect too much from the sources. Watson has warned us that the complete story was not told and that he was "forced to improvise the details." But we should expect to find some evidence of the mysteriousness of the Koshima events of 1958. In particular, we should expect to find evidence of an episode of sudden learning within the troop at this time (though perhaps not in one afternoon) and evidence of the sudden appearance of potato washing in other troops sometime soon after the Koshima event. We also have a negative expectation of the literature; it should *fail* to report certain important details. It will not (we expect) tell us the exact number of monkeys washing potatoes prior to or after the event of 1958, nor will it provide us with an explanation of how the post-event Koshima learners were able to acquire their knowledge. After all, it is Watson's claim that the event produced *paranormal* learning of potato washing. These three expectations will be tested against the literature. Was there a sudden event at Koshima? Did acquisition at other colonies follow closely the Koshima event? Does Watson improvise details *only* when the cited literature fails to provide adequate information? The following comments will be restricted to the literature on macaques actually cited by Watson.

Almost all of the information about the Koshima troop appears in a journal article by Masao Kawai (1965); the other articles are secondary on this topic. Kawai's article is remarkably detailed in its description of the Koshima events. The troop numbered 20 in 1952 and grew to 59 by 1962. (At least in the numerical sense, there was never a "hundredth monkey" on Koshima.) Watson states that "an unspecified number" of monkeys on Koshima had acquired the potato-washing habit by 1958. Actually this number was far from unspecified. Kawai's data allowed the reader to determine the dates of acquisition of potato washing (and two other food behaviors), as well as the dates of birth and geneological relationships, *of every monkey in the Koshima troop from 1949 to 1962* (Figure 1, pp. 2-3, and elsewhere in the paper). In March 1958, exactly 2 of 11 monkeys over 7 years old had learned potato washing, while exactly 15 of 19 monkeys between 2 and 7 had the habit (p. 3). This amounts to 17 of 30 noninfant monkeys. There is no mention in this paper (or in any other) of a sudden learning event in the fall of 1958. However, it is noted that by 1962, 36 of the 49 monkeys had acquired the habit. So both the population and the number of potato washers had increased by 19 during this four-year period. Perhaps this is what suggested to Watson that a sudden event occurred in the fall of 1958. And perhaps (since one can only surmise) this idea was reinforced in Watson's mind by the following statement by Kawai: "The acquisition of [potato washing] behavior can be divided into two periods; before and after 1958" (p. 5).

So Kawai does not give a time of year, a day of the week, or even the season for any sudden event in 1958. But he does at least identify the year.

And is Kawai mystified about the difference between pre- and post-1958 acquisition? Is he "not quite sure what happened"? Is he reluctant to publish details "for fear of ridicule?" No. He publishes the whole story, in gothic detail. The post-1958 learning period was remarkable only for its normalcy. The period from 1953 to 1958 had been a period of exciting innovation. The troop encountered new food sources, and the juveniles invented ways of dealing with these sources. But by 1958 the innovative youth had become status quo adults; macaques mature faster than humans. The unusual juvenile-to-adult teaching methods reverted to the more traditional process of learning one's food manners at one's mother's knee. Imo's first child, a male named "Ika," was born in 1957 (pp. 5, 7). Imo and her former playmates brought up their children as good little potato-washers. One can only hope that Ika has been less trouble to his Mom than Imo was to hers. Kawai speaks of the innovative period from 1953 to 1958 as "individual propagation" (p. 5) and the period after 1958 as "pre-cultural propagation" (p. 8). (This latter term does not indicate anything unusual for the monkey troops. The troops under normal circumstances have behavioral idiosyncrasies and customs that are passed along within the group by "pre-cultural" means. The expression only indicates a reluctance to refer to monkey behavior as genuinely "cultural.")

So there was nothing left unsaid in Kawai's description. There was nothing mysterious, or even sudden, in the events of 1958. Nineteen fifty-eight and 1959 were the years of maturation of a group of innovative youngsters. The human hippies of the 1960s now know that feeling. In fact 1958 was a singularly poor year for habit acquisition on Koshima. Only two monkeys learned to wash potatoes during that year, young females named Zabon and Nogi. An average of three a year had learned potato washing during the previous five years (Table 1, p. 4). There is no evidence that Zabon and Nogi were psychic or in any other way unusual.

Let us try to take Watson seriously for a moment longer. Since only two monkeys learned potato washing during 1958 (according to Watson's own citation), one of them must have been the "Hundredth Monkey." Watson leaves "unspecified" which monkey it was, so I am "forced to improvise" and "say, for argument's sake" that it was Zabon. This means that poor little Nogi carries the trim metaphysical burden of being the "almost everyone in the colony" who, according to Watson, suddenly and miraculously began to wash her potatoes on that autumn afternoon.

Watson claims that the potato-washing habit "spontaneously" leaped natural barriers. Is there evidence of this? Well, two sources report that the behavior was observed off Koshima, in at least five different colonies (Kawai 1965, 23; Tsumori 1967, 219). These reports specifically state that the behavior was observed only among a few individual monkeys and that it had not spread throughout a colony. There is no report of when these behaviors occurred. They must have been observed sometime between 1953 and 1967. But there is nothing to indicate that they followed closely

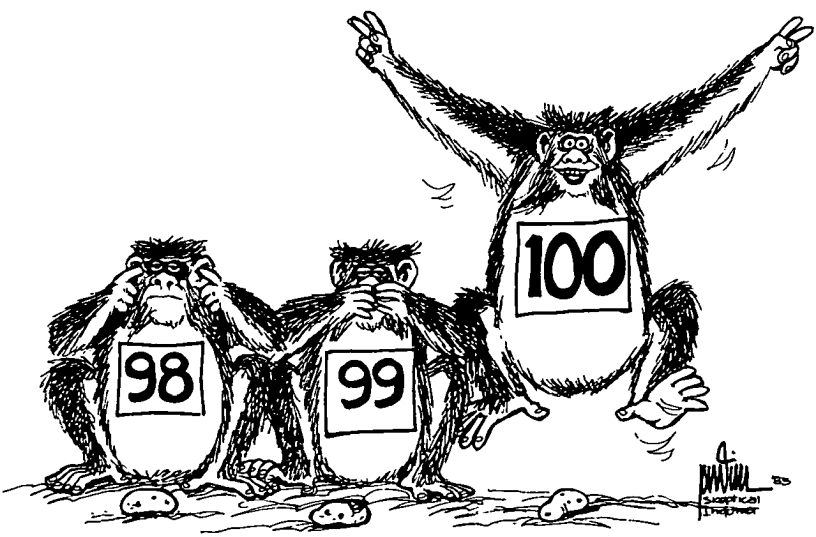
upon some supposed miraculous event on Koshima during the autumn of 1958, or that they occurred suddenly at any other time, or that they were in any other way remarkable.

In fact there is absolutely no reason to believe in the 1958 miracle on Koshima. There is every reason to deny it. Watson's description of the event is refuted *in great detail* by the very sources he cites to validate it. In contrast to Watson's claims of a sudden and inexplicable event, "Such behavior patterns seem to be smoothly transmitted among individuals in the troop and handed down to the next generation" (Tsumori 1967, 207).

## Methodology of Pseudoscience

The factual issue ends here. Watson's claim of a "Hundredth Monkey Phenomenon" is conclusively refuted by the very sources he cites in its support. He either failed to read or misreported the information in these scientific articles. But Watson's own mode of reasoning and reporting, as well as the responses he has inspired in the popular literature, deserve attention. They exemplify the pseudoscientific tradition. Consider the following:

1. Hidden sources of information: Watson informs us that the scientific reports leave important data "unspecified." This is simply false. But, more subtly, he tells us that most of the researchers are still unsure of what happened and that those who "do suspect the truth are reluctant to publish it for fear of ridicule." In one fell swoop Watson brands himself as courageous, explains why no one else has dared report this miraculous phenomenon, and discourages us from checking the cited literature for



corroboration. Watson got the real story from "personal anecdotes and bits of folklore among primate researchers. . . ." Those of us who don't hobnob with such folks must trust Watson. The technique was effective. Of the commentaries I have found on the Hundredth Monkey Phenomenon, not one shows evidence of having consulted the scientific sources cited by Watson. Nonetheless, each presents Watson's fantasy as a scientifically authenticated fact. Nor is additional information available from Watson. I have written both to Watson and to his publishers requesting such information and have received no reply.

2. Aversion to naturalistic explanations: The fact is that potato washing was observed on different islands. Watson infers that it had traveled in some paranormal way from one location to another. Like other aficionados of the paranormal, Watson ignores two plausible explanations of the concurrence of potato washing. First, it could well have been an independent innovation—different monkeys inventing the same solution to a common problem. This process is anathema to the pseudoscientist. The natives of the Americas simply *could not have* invented the pyramid independent of the Egyptians—they just didn't have the smarts. In more extreme cases (von Däniken, for example) a *human being* is just too dumb to invent certain clever things—extraterrestrials must have done it.

Watson assumes that Imo was the only monkey capable of recognizing the usefulness of washing potatoes. In his words, Imo was "a monkey genius" and potato washing is "comparable almost to the invention of the wheel." Monkeys on other islands were too dumb for this sort of innovation. But keep in mind that these monkeys didn't even *have* potatoes to wash before 1952 or 1953, when provisioning began. Monkeys in at least five locations had learned potato washing by 1962. This suggests to me that these monkeys are clever creatures. It suggests to Watson that *one* monkey was clever and that the paranormal took care of the rest. A second neglected explanation is natural diffusion. And indeed Kawai reports that in 1960 a potato washer named "Jugo" swam from Koshima to the island on which the Takasakiyama troop lives. Jugo returned in 1964 (Kawai 1965, 17). Watson does not mention this. The Japanese monkeys are known to be both clever and mobile, and either characteristic might explain the interisland spread of potato washing. Watson ignores both explanations, preferring to invent a new paranormal power.

3. Inflation of the miracle: As myths get passed along, everyone puffs them up a bit. The following two examples come from second-generation commentaries that quote extensively from Watson. Nevertheless, even Watson's claims are beginning to bulge. First, the primatologists' reports had mentioned that only a few isolated cases of off-Koshima potato-washing were observed. Watson reports this as the habit's having "appeared spontaneously . . . in colonies on other islands. . . ." Not actually false, since the few individuals were indeed *in* other colonies (though only individuals and not whole colonies adopted the behavior). Following Watson,

Ken Keyes reports that, after the hundredth Koshima monkey, "colonies of monkeys on other islands . . . began washing their sweet potatoes"! (Keyes 1982, p. 16). From Keyes, one gets the image of spontaneous mass orgies of spud-dunking. A second example: Regarding the primatologists' attitudes toward the events of 1958, Watson reports only that they are "still not quite sure what happened." But the primatological confusion quickly grows, for *Science Digest* (1981) reports "a mystery which has stumped scientists for nearly a quarter of a century." In these two particular cases, Watson's own statements are at least modest. They're not what one would call accurate, but not exorbitantly false either. By the second generation we find that "not quite sure what happened" becomes "stumped for nearly a quarter of a century," and the habit that *appeared in* individuals within colonies of monkeys becomes a habit *of* colonies of monkeys. Please keep in mind that the second generation relies *only* on Watson for its information; even Watson's none-too-accurate report has been distorted—and not, needless to say, in the direction of accuracy.

4. The paranormal validates the paranormal: The validity of one supernatural report is strengthened by its consistency with other such reports. Watson's commentators show how this works. Keyes supports the Hundredth Monkey Phenomenon by its consistency with J. B. Rhine's work at Duke, which "demonstrated" telepathy between individual humans. "We now know that the strength of this extrasensory communication can be amplified to a powerfully effective level when the consciousness of the 'hundredth person' is added" (Keyes 1982, 18). Elda Hartley's film "The Hundredth Monkey" invokes Edgar Cayce. And in a remarkable feat of group consciousness, *four of the five* secondary sources emphasize the similarities between Watson's Hundredth Monkey Phenomenon and Rupert Sheldrake's notion of the "morphogenetic field." The spontaneous recognition of the similarities between Watson and Sheldrake seems to have leaped the natural boundaries between the four publications! Now *there's* a miracle! (Surely independent invention or natural diffusion couldn't account for such a coincidence.)

## Conclusions

I must admit sympathy for some of the secondary sources on the Hundredth Monkey Phenomenon. This feeling comes from the purpose for which the phenomenon was cited. Ken Keyes's book uses the phenomenon as a theme, but the real topic of the book is nuclear disarmament. Arthur Stein's article and (to a lesser extent) the Hartley film are inspired by Keyes's hope that the Hundredth Monkey Phenomenon may help prevent nuclear war. The message is that "you may be the Hundredth Monkey" whose contribution to the collective consciousness turns the world away from nuclear holocaust. It is hard to find fault in this motive. For these very same reasons, one couldn't fault the motives of a child who wrote to

Santa Claus requesting world nuclear disarmament as a Christmas present. We can only hope that Santa Claus and the Hundredth Monkey are not our best chances to avoid nuclear war.

Watson's primary concern is not prevention of war but sheer love of the paranormal. His book begins with a description of a child who, before Watson's own eyes, and with a "short implosive sound, very soft, like a cork being drawn in the dark," psychically turned a tennis ball inside out—fuzz side in, rubber side out—without losing air pressure (p. 18). Just after the Hundredth Monkey discussion, Watson makes a revealing point. He quotes with approval a statement attributed to Lawrence Blair: "When a myth is shared by large numbers of people, it becomes a reality" (p. 148). This sort of relativist epistemology is not unusual in New Age thought. I would express Blair's thought somewhat differently: "Convince enough people of a lie, and it becomes the truth." I suggest that someone who accepts this view of truth is not to be trusted as a source of knowledge. He may, of course, be a marvelous source of fantasy, rumor, and pseudoscientific best-sellers.

I prefer epistemological realism to this sort of relativism. Truth is not dependent on the numbers of believers or on the frequency of published repetition. My preferred epistemology can be expressed simply: Facts are facts. There is no Hundredth Monkey Phenomenon.

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# The Responsibilities of the Media and Paranormal Claims

*Because the media are a dominant influence in the growth of belief in the paranormal, there is a need to develop among journalists an appreciation for critical judgment in evaluating claims of truth.*

Paul Kurtz

THE COMMITTEE FOR the Scientific Investigation of Claims of the Paranormal was founded when a number of scientific skeptics and rationalists, increasingly concerned about the rising tide of unchallenged paranormal claims, decided to form a coalition of individuals committed to the use of science and rational methods of inquiry in evaluating such claims.

The word *paranormal* was being loosely used (we did not invent the term) to include many diverse things under its rubric; everything from psychic prophecies, ESP, clairvoyance, telepathy, psychokinesis, apparitions, hauntings, poltergeists, communication with discarnate spirits, reincarnation, levitation, psychic healings, on the one hand, to astrological charts and horoscopes, UFO sightings and abductions, Bermuda Triangles, and monsters of the deep, on the other. We thought it incredible that so many films, TV and radio programs, news stories, and books were presenting these paranormal claims as the gospel truth, even maintaining that they had been proven by science, and that there was little or no public awareness of the fact that when these claims were subjected to careful scientific appraisal they were shown to be either unverified or false.

We found the paranormal field so rife with wishful and exaggerated claims that we felt the public should have the opportunity to learn about dissenting scientific studies and thus have a more balanced picture. With this in mind, we launched CSICOP and a new journal, *The Zetetic,* which after the first two issues became the SKEPTICAL INQUIRER. We were

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committed to giving an impartial hearing to claims of the paranormal and to making our findings available to the general public.

CSICOP was and still remains a grassroots movement. Little did we imagine when we began that we would grow so rapidly, that we would stimulate the formation of affiliated groups in ten other countries throughout the world, that local and regional groups would spring up all over the United States, and that the SKEPTICAL INQUIRER, under the masterful editorship of Kendrick Frazier, would reach close to 23,000 circulation and still be growing rapidly. Nor did we anticipate the attention we would draw—pro and con—from the media, the public, and our fellow scientists. We apparently have crystallized a deep-felt need, particularly in the academic and scientific community.

We are today witness to a breathtaking scientific and technological advance. Yet the scientific revolution is a relatively new development. Since its beginning in the sixteenth century, it has transformed the globe and radically altered human society. It led to the industrial revolution and the development of electronic and computer technology. It has given us the conveniences of modern life, made rapid communications and travel possible, including the great adventure into space. Scientific progress has dramatically increased food production, enabled us to control many diseases, and has contributed enormously to the betterment of the human life. For large sectors of the population, science is simply equated with the latest technological innovations, which are gladly accepted because of their economic benefits.

Unfortunately, the public is often unaware of, or does not accept, other equally significant aspects of the scientific revolution. First there is the challenge of new intellectual conceptions of nature. Our planet is no longer the center of the universe, whose estimated age and size have been expanded tremendously. All life forms, including the human species, are part of nature, not separate from it, and have evolved over a long period of time. Our perspectives on nature and life are constantly being revised as the frontiers of science grow. Yet, in spite of this, ancient primitive, animistic, mythological, and occult views still prevail.

Second, and often overlooked by the public, is the fact that modern science was made possible by the development of powerful new methods of inquiry. Although techniques and procedures may vary from field to field and subtle social and psychological factors intervene in the development of science, still the process of scientific investigation emphasizes certain basic methodological criteria: (a) the use of experimental methods for testing hypotheses, the insistence upon verification, prediction, and replication, and (b) the use of deductive inferences in validating mathematical and theoretical constructs and in explaining the observed data.

Intrinsic to scientific investigation is some element of skepticism. This means that we need to pay diligent attention to the facts, including novel, discordant, and anomalous data that do not fit into the existing parameters

of explanations. One cannot rely on an appeal to the authorities. However well-respected they may be, they may be mistaken. Thus intrinsic to science is the self-corrective process whereby earlier hypotheses and theories are revised in the light of new data and new explanations. Scientific knowledge cannot be taken as absolutely fixed or final. Although there may be fairly reliable degrees of certainty, it is in the last analysis only hypothetical, tentative, probable. Unorthodox claims to truth may turn out to be correct in the end; they must not be peremptorily rejected. Before hypotheses can be accepted, however, they must be subjected to rigorous scrutiny by a community of objective inquirers.

It is puzzling that, given the tremendous boon the scientific revolution has been to world civilizations, many people have failed to understand its implications regarding the formulation of new conceptual outlooks, its unsettling effect on old faiths and beliefs based on prescientific prejudice and habits, and the importance of the scientific method in testing truth claims. Perhaps this is one of the reasons there still persist in contemporary society so many occult, spiritualistic, and paranormal notions of reality.

What can we do to correct this deficiency? Let me identify some directions I submit we should take. First, I think it is an important task of education to convey to students these two senses of science. However, it is not simply science as it is currently taught. Today science is taken as a technical specialty, and graduates of professional schools are credentialed primarily for their expertise in their chosen fields. Alas, as Emerson observed long ago, we are training men and women who know only a small corner of the spectrum of knowledge and are largely ignorant of the broader implications—and, I might add, do not know how to apply the methods of critical scientific inquiry to fields outside of their own disciplines. The Institute for Creation Research, for example, has several hundred chemists, engineers, and other specialists with advanced degrees affiliated with it, no doubt competent in their own subjects yet unable to apply the methods of critical scientific intelligence beyond their fields of expertise. I would suggest that it is not enough to train narrow specialists and technicians; both science and nonscience majors must be exposed to general education, and in particular science needs to be understood as one of the creative and liberating arts. We need to ask, Can the general methods of scientific inquiry serve as a model for other fields of human interest? Philosophers have explored the possibilities of extending the tentative evidential and rational approach used in science to other areas in which we formulate our beliefs and evaluate claims to truth. Today we are confronted by various forms of anti-intellectualism, even among college graduates, that abandon any pretense to objective, reflective, or critical inquiry and substitute faith, subjective prejudice, or occult thinking.

We are also faced with the dominant influence of the media in forming attitudes and beliefs; for the growth of widespread belief in paranormal, pseudoscientific, and other untested claims may be traced in large measure

to the distorted presentation that appears on television, in films, and in print. In other words, many people accept psychic, astrological, or UFOlogical reports as true because of what they have seen, heard, or read in the media. Much misinformation and exaggeration can be traced to the desks of editors, journalists, publishers, program directors, and film producers.

One question often raised is, How shall people in the scientific and academic community respond to the challenge of paranormal claims? The response should be, first and foremost, "By scientific research." In other words, what we need is open-minded, dispassionate, and continuing investigation of claims and hypotheses in the paranormal realm. Here one must be fair-minded and one should work cooperatively—as Marcello Truzzi has pointed out—with the so-called paranormalists. The dogmatic refusal to entertain the possibility of the reality of anomalous phenomena has no place in the serious scientific context. The hypotheses and data must be dealt with as objectively as possible, without preconceived ideas or prejudices that would mean the death of the scientific spirit. We cannot reject unconventional or outrageous ideas simply because they are unfamiliar or upsetting to our existing theories. They may after all turn out to be true. Their proof or disproof is found by doing the hard work of scientific investigation.

Unfortunately, the paranormal realm does not always lend itself to a dispassionate withdrawal into the quiet laboratory or library. For the paranormal is of such vital public interest that it immediately becomes news. Reporters are constantly sniffing at the heels of the parapsychologists and are ever-ready to take the most slender shred of evidence or the mere inkling that something may be true, inflate it out of proportion to its tentative epistemological status, and proclaim it as proven scientific dictum. In the paranormal realm, tentative hypotheses are readily converted into proven truths by overzealous reporters more interested in entertaining the public than in providing accurate information.

The media often behave totally irresponsibly in treating "paranormal" occurrences. This can be illustrated by two cases concerning paranormal powers that surfaced in the past year and were given a great deal of media attention. The first concerns the Columbus, Ohio, poltergeist (*SI,* Summer 1984 and Spring 1985), and the second, the reports of a psychic arms race.

In the first case, Tina Resch, a fourteen-year-old emotionally disturbed adolescent, a school dropout, and the adopted daughter of John and Joan Resch, became the central figure. The drama began when lights started to go off and on mysteriously in the Resch home and objects flew about whenever Tina was present. Finally a reporter, Mike Harden, and a photographer, Fred Shannon, both from the *Columbus Dispatch,* appeared at the house. Shannon took several photographs of a phone flying through the air (later called "the attack of the killer phone"). The phone would not take off, he reported, when he had his camera directly on it; the "force"

eluded him. But he got the best of it by turning his head and peeking out of the corner of his eye. Then, as he glimpsed it flying through the air, he turned and got a shot. Mike Harden backed him up, maintaining that there was no known explanation for why phones, eggs, knives, cups, and lamps were flying all over; only a paranormal one seemed to suffice.

Shannon's photograph and Harden's story hit the newspapers worldwide; many even front-paged the photo. "Poltergeists!' they said, in banner headlines. At this point, CSICOP was besieged by the media: What was our view? We questioned most of the principals. We even sent a three-man team (Randi and Professors Shore and Sanduleak) to Columbus to do field research, but they were refused admittance to the house by the Resches.

After extensive analysis, we concluded that Tina cheated. There were TV tapes to prove it. She was seen knocking down a lamp when she thought no one was watching and when she was unaware that the tape was rolling. When confronted with this evidence, Tina admitted trickery but said that she was tired and just wanted the news crews to leave the house. Incredibly, this explanation was readily accepted by Harden and other reporters and, even though the TV tape was shown on the Columbus newscast, camera crews and the news services continued to spew out a steady flow of poltergeist stories.

The Resch family permitted a parapsychologist from Durham to enter the house and to take Tina back to North Carolina for further testing. The parapsychologist, William Roll, admits that Tina cheated but also believes that she has genuine "psychokinetic energies" and that he observed objects flying through the air.

We in CSICOP who investigated the case are convinced that the entire affair was based on sleight of hand and fraud—and an analysis of the Shannon photographs confirms this interpretation. Tina has since admitted that she had seen the film *Poltergeist* many times; she also claims to have "healing powers." She has subsequently been shown to have cheated on other occasions by throwing things in the air when no one was looking. Yet the *Columbus Dispatch* has continued to give the paranormal interpretation. Indeed, the editor refused to give the SKEPTICAL INQUIRER permission to print the original photographs with the exposé of the case written by James Randi. Whichever interpretation is correct, in our view at least, the media, and especially the *Columbus Dispatch,* behaved outrageously. There is one notable exception in this case: Davyd Yost, a reporter on the rival Columbus newspaper, the *Citizen Dispatch,* provided a balanced analysis of the case to readers in his area.

The second media buildup concerns the so-called psychic arms race, which was reported during the past year by news sources as diverse as Jack Anderson, *Time,* the Associated Press, and even the *New York Times.* The claim is that "remote viewing" and "psychokinesis" have been decisively demonstrated in the laboratory and that psychics have been

successfully used to locate enemy submarines and missile bases. Moreover, it is alleged that both the United States and the Soviet Union are competing in such research. What are we to make of this? Three books were published in the past year touting the use of psychics by the military. Has there been balanced reporting? Unfortunately, sensationalism has been the rule rather than the exception, and the so-called psychic arms race is largely a media event exaggerated out of all proportion to the actual facts.

Other cases of blatant misreporting arise all too frequently, such as the many stories on the Hudson Valley UFO sightings, which were reported on at length in the November 1984 *Discover* magazine as a hoax. But this was a notable exception to the many early stories that had heralded the phenomena as genuine extraterrestrial visitors.

CSICOP believes in freedom of expression. We do not believe in censorship. We ask only that those charged with supplying our news exert some form of responsibility and perhaps skepticism in reporting it, particularly when issues of scientific accuracy are at stake. We ask only for balanced reporting.

In conclusion, we need to pay serious attention to the way our journalists are educated, particularly in the area of science reporting, and how they are selected for their important positions. The media are so central to contemporary life that only the highest standards of selection and peer review should apply; the same rigorous standards that we use in selecting doctors, lawyers, and professors need to be employed in selecting our reporters and radio talkshow moderators. We have no desire to limit the discussion but only to improve its quality.

Here, of course, the colleges and universities have a vital role in educating future journalists. Some writers appear to be illiterate in even the most elementary understanding of science and of the means of evaluating truth and falsity. They ignore the standards of responsible journalism and often sensationalize the news to ensure conspicuous placement of their stories. This indictment does not apply to many or most science writers or to the many experienced journalists who recognize their responsibility to report accurately, and I don't mean it as a blanket indictment of everyone in the media. But it does apply to all too many journalists and reporters.

I should add that the problem of media misinformation also applies to politics, business, religion, ethics, medicine, and other topics of vital social concern. CSICOP is primarily interested in the reporting of the paranormal and pseudosciences, but what happens in these areas is perhaps symptomatic of the broader problem—the need to develop in both the journalistic fraternity and the public some sort of appreciation for critical and reflective judgment in evaluating claims of truth. This need is made more critical by the fact that the public is constantly being bombarded by those who wish to promote their own views, sell a bill of goods, convert others to a cause, or convince us that they have discovered a special truth or have found a unique road to salvation. •

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# 'Lucy' Out of Context

## *The "scientific" creationists continue to use one of their favorite and most devious tactics to deal with embarrassing evidence.*

Leon H. Albert

VIRTUALLY EVERY critic of "scientific" creationism has commented on the apparently unfailing propensity of its advocates to use out-of-context quotations to support their various arguments against evolution. The out-of-context quotation constitutes one of the high sins of legitimate scholarship. Indeed, even "scientific" creationists, when they abandon their pretense of being legitimate scientists and engage in their more customary activity of finding biblical support for their various fundamentalist theological stances, are fond of contending that it is unacceptable to cite biblical passages out of context (a rule more often broken than observed).

In its most basic form, the out-of-context quotation consists of the use of an authority's words, ripped from their context, in order to support a point that those words, when taken in context, do not support and may even contradict. There are a number of variations in the use of this tactic, practically all of which are to be found in the "scientific" creationists' arsenal. The one I will focus on is the use of an authority's published words against evidence that was *not even available* at the time of the publication of those words. This too constitutes a form of out-of-context quotation precisely because the context within which the authority was writing did not include the evidence currently at issue.

The use of such a tactic reflects a basic dishonesty on the part of those who resort to it. They seek to leave their audience with the impression that the cited authority supports their interpretation of the evidence,

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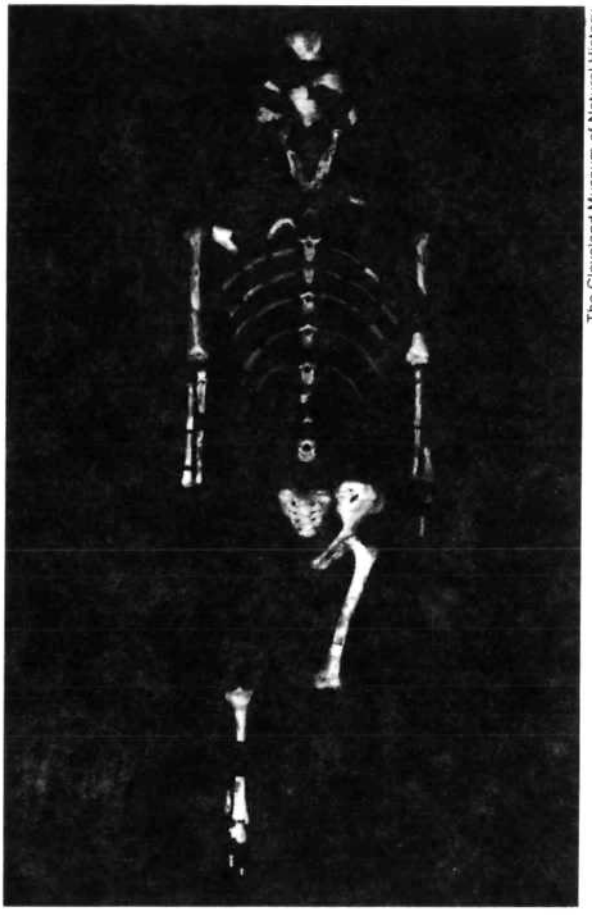


The Cleveland Museum of Natural History

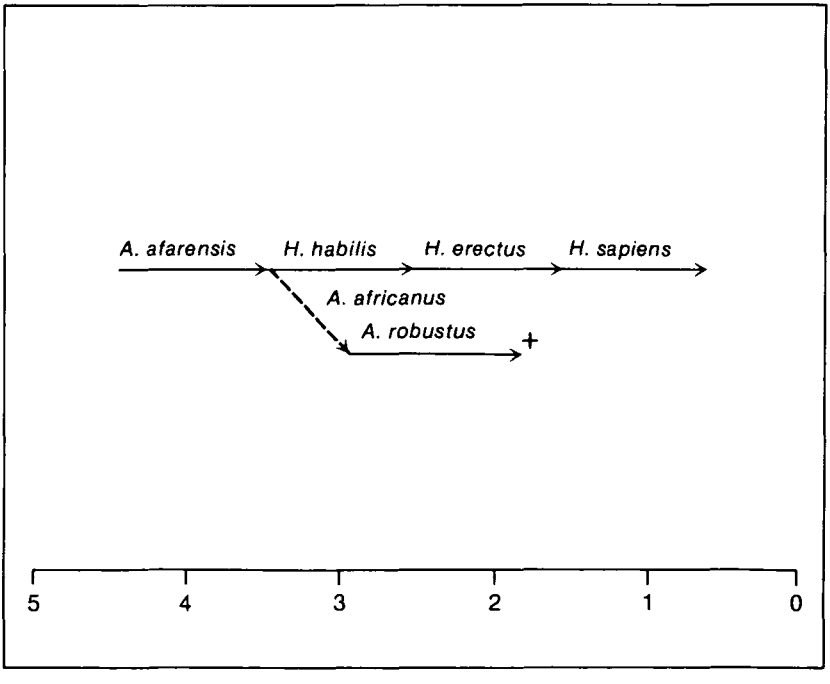
"Lucy": 40% complete female skeleton of *Australopithecus afarensis*.

when in fact that authority was not even addressing that evidence. This subterfuge is maintained through a careful avoidance of any mention of the date either of the authority's words or of the availability of the evidence whose interpretation is at issue.

The specific evidence I will address here is a group of 3.5-million-year-old hominid fossils that have come to be identified with their most famous representative, the fossil known as "Lucy." Lucy and numerous similar fossils discovered in northeastern and eastern Africa are now generally recognized as a distinct form of early hominid (i.e., members of the human family), sufficiently different from any previously named category to place them in their own taxon, or category, *Australopithecus afarensis*. *A. afarensis* was basically an apelike creature in all its characteristics, with one extremely important exception: *A. afarensis* walked, if not in the completely upright manner characteristic of modern humans, at least in a bipedal manner that is not found in any existing apes. Such

bipedal locomotion has long served as a major anatomical criterion in distinguishing humans and their ancestors from apes and their ancestors. We must always recognize the possibility that such a mode of locomotion could have arisen more than once during the course of primate evolution.

Prior to the discovery of these fossils in 1974 and the publication of a description of them in January 1979 (please note the dates), justifying the creation of a new taxon by Johanson and White, most anthropologists had recognized only two members of the genus *Australopithecus.* These were the species *A. africanus* and *A. robustus.* Many anthropologists seem to agree with the interpretation offered by Johanson and White regarding the evolutionary relationships between these various forms. They suggest that, after Lucy-type forms gave rise to the line leading to the *africanus* and the subsequent *robustus* forms, this particular evolutionary line went on to extinction without making any contribution to hominid evolution. Meanwhile, they argue, another descendant of a Lucy-type form, known as *Homo habilis,* was ancestral to the line that eventually gave rise to ourselves (see Fig. 1). This line was characterized by an increasing brain size.




After Johanson & White

FIGURE 1

This particular interpretation of the evolutionary relationships between these various forms must be viewed within the context of the often heated debate that occurred in anthropology prior to the discovery of *Australopithecus afarensis.* This debate centered on the question of the taxonomic status of the first known forms of *Australopithecus:* Were they to be classified as hominids (i.e., within the human family) or as pongids (i.e., within the ape family)? The current consensus seems to be that, while the forms *A. africanus* and *A. robustus* were indeed hominids, they were not ancestral to ourselves but were instead a sideline that became extinct.

Confronted with the fossil form *Australopithecus afarensis* (i.e., Lucy and other fossils of the same kind), "scientific" creationists have taken to citing early authoritative criticisms (now generally confirmed) against the once-claimed prehuman status *of the other two forms* of *Australopithecus* (i.e., *A. africanus* and *A. robustus*) as legitimate evidence against assigning a hominid or prehuman status to Lucy (*A. afarensis*) and her kind.

Creationists have sought to accomplish this particular bit of obscurantism through an apparent intentional avoidance of any recognition of differences between the various forms of *Australopithecus.* In this way, authoritative comments originally meant to apply to only one or two particular forms are dishonestly applied to yet another.

By far the most popular sources among "scientific" creationists of such authoritative retroactive criticisms of the hominid status of Lucy-type fossils are, first, a book by Sir Solly Zuckerman, *Beyond the Ivory Tower,* written in 1970, four years before the discovery of Lucy and nearly a full ten years before the publication of the description of the Lucy fossil. The second such source is the article "Human Fossils: New Views of Old Bones," by Charles Oxnard, published in May 1979 but, according to Oxnard (personal communication), written prior to the publication of Johanson and White's article of January 1979. It is these two works, both questioning the prehuman status of *A. africanus* and *A. robustus* and both written *prior* to the publication of Johanson and White's article describing the nature of the *Australopithecus afarensis* fossil material, that are retroactively used in the "scientific" creationists' attacks against the claimed prehuman status of *Australopithecus afarensis.*

Thus, in a debate with Kenneth Miller at Brown University on April 10, 1981, Henry Morris, president of the Institute for Creation Research and the high guru of "scientific" creationism, claimed, *with explicit reference to the "Lucy" fossil,* that both Zuckerman and Oxnard "have analyzed this material in detail, measured it in the laboratory . . . and they have taken all of the australopithecine dentition, the skull structure that has been found, the limb bones, the knee bones, and so forth, and they have come to the conclusion that . . . it looks like an ape" (transcribed from an audio tape of the debate).

One can only wonder just how it was that Zuckerman and Oxnard could have analyzed the Lucy material in detail "in the laboratory" before

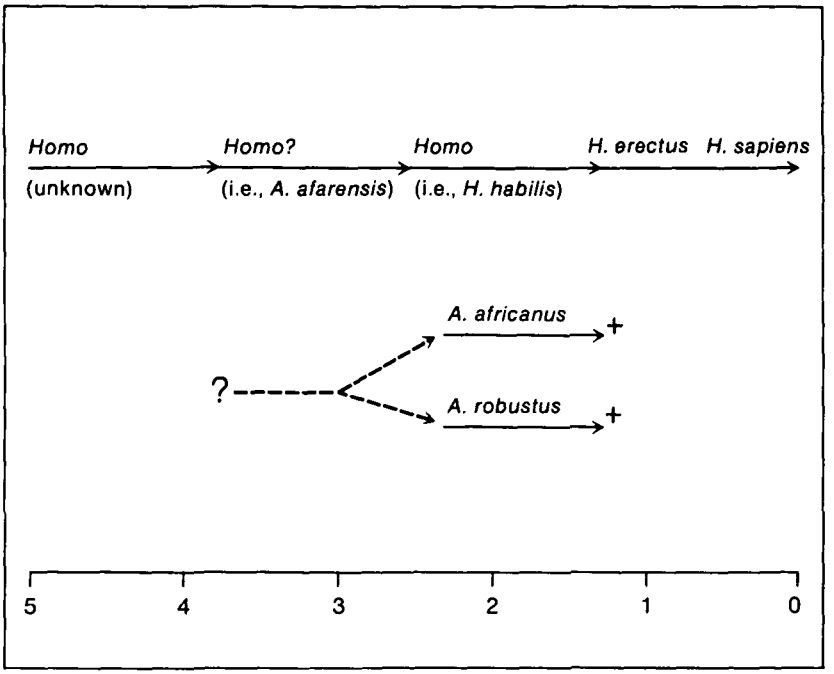
its discoverers had even published a formal description of that material. Moreover, Zuckerman's amazing ability to accomplish this feat a full four years before the fossil had even been discovered is particularly intriguing. In fact, neither Zuckerman nor Oxnard have studied the material classified as *A. afarensis* (Oxnard, personal communication). How could they? The material was simply not available to either of them.

A couple of months after the Brown University debate, in the June 1981 *Acts & Facts,* a monthly publication put out by the Institute for Creation Research under the editorship of Henry Morris, in an article entitled "Summary of Scientific Evidence for Creation," we find Lord Zuckerman's 1970 book being cited once again. The article, written by Duane Gish and Richard Bliss, and reviewed by the creationist attorney Wendell Bird, claims there are no fossil traces of a transformation from an apelike creature to man. Conveniently ignored is the fact that Zuckerman made this claim long before the discovery of the now plentiful remains of *Australopithecus afarensis!*

Incidentally, in this same article, pages 79-94 of Zuckerman's book are cited in support of the creationist contention that a yet earlier proposed transitional form known as *Ramapithecus* is now supposedly known to be fully apelike. The pages referred to contain not a single word regarding *Ramapithecus.* In fact, current research on recently discovered *Ramapithecus* remains from China indicates that, while these creatures were unique in many ways, they often show a greater resemblance to humans than to apes. Ironically, this conclusion is the result of original research carried out in part by, of all people, Charles Oxnard (Rukang and Oxnard 1983). Needless to say, such findings will be ignored by "scientific" creationists.

Gish and Bliss also cite Oxnard's article in support of their contention that "*Australopithecus,* in the view of some leading evolutionists, was not intermediate between ape and man and did not walk upright." At no place in his article does Oxnard contend that *Australopithecus* did not walk upright. Indeed, at one point he explicitly concedes that "they may well have been bipeds," even if not in the "human manner." Thus, Gish and Bliss have attributed to Oxnard not only a claim that he does not make but one that stands in direct contradiction to his clearly expressed opinion. There is simply no nice way to say it—Gish and Bliss are simply flat-out lying. There is virtually no way in which the concession that the australopithecines "may well have been bipedal" can be creatively reinterpreted to mean that they "did not walk upright."

As for the claim that Oxnard does not consider *Australopithecus* to be intermediate between apes and humans, Gish and Bliss blur over the distinctions between the various forms of *Australopithecus.* They neglect to inform their readers that this judgment of Oxnard's was meant to apply *only* to the forms *A. africanus* and *A. robustus* and most definitely not to the form subsequently known as *A. afarensis.* This is quite clearly indicated in Oxnard's article, which explicitly refers to the "new fossil




After Oxnard

FIGURE 2

finds" of Johanson and others and expressly identifies this material as humanlike. Moreover, Oxnard places this new fossil material on the ancestral line of humans in the evolutionary tree included in his article (see Fig. 2). And, although he himself identifies this new material as *"Homo,"* he plainly states that it will probably be classified among the australopithecines. He even warns his readers to be alert to this likely change in names. Needless to say, Morris, Gish, and Bliss studiously avoid any reference to this warning when they cite Oxnard.

In sum, Gish and Bliss, like Morris, are guilty of reading opinions into Oxnard's article that not only are not there but are actually in direct contradiction to what is there. In order to achieve this, they have been dishonest both with themselves and with their readers.

While these scholarly felonies were committed back in 1981, Gish continues to use these tactics. The December 1983 *Acts & Facts* gives the following account of a debate between Gish and Steven Shore at Case Western Reserve University in Cleveland:

> Shore claimed that the pelvis demonstrated that "Lucy" and associated

creatures walked upright, and therefore these ape-like creatures were on their way to becoming people. Gish, using the date of Lord Zuckerman and Dr. Charles Oxnard had earlier stated that both Lord Zuckerman and Dr. Oxnard had established that the australopithecines did not walk upright and were not intermediate between ape and man. Gish merely appeared amused by the tactics of Shore and reiterated Zuckerman's and Oxnard's conclusions concerning the australopithecines.

In light of the foregoing, I suggest that Gish's apparent amusement was similar to that of the confidence man who believes he has pulled yet another fast one on his unsuspecting marks.

We find still another instance of this particular quotation out of context in one of the latest diatribes against evolution produced by the Institute for Creation Research. This is the book *What is Creation Science?* (1983). The first half is written by Gary E. Parker, and the second half by Henry M. Morris.

In the caption accompanying an illustration of the three forms of *Australopithecus,* Parker misinforms the reader that the "australopithecines, including Johanson's 'Lucy,' . . . are current candidates for man's ancestors" (1983:123). He then goes on to say that Oxnard, who we are told in the body of the text has been able to "examine the evidence" (1983:121), has concluded that "if the australopithecines walked upright, it was not in the human manner." Thus the reader is left with the totally false impression that Oxnard was speaking about the Lucy fossil when in fact his comments were directed only to a fossil of the *africanus* type.

Parker goes even further in his misrepresentation of Oxnard's work. To appreciate this misrepresentation, a little background is necessary. In his 1979 paper, Oxnard describes a three-dimensional model, resembling a ball-bond model of molecular structure, that he formulated in 1975 to illustrate the anatomical similarities and differences between modern humans, modern apes, and *A. africanus.* Using a technique known as multivariate analysis, he focused on a comparison of a specific *toe bone* in these different forms. Oxnard concluded that if this *model* was viewed from one angle, it did indeed indicate a close relationship between humans and australopithecines. (Remember, at the time of his writing, Lucy and her kind had not yet been formally identified as members of the genus *Australopithecus,* and Oxnard did not use this material in constructing his model.) However, he pointed out, if the model was viewed from another angle, it showed the fossil toe to be "as far distant from modern man as are the African apes" (1979:268).

Parker, either through conscious deception or through incredibly inept and sloppy scholarship, seeks to create the impression that Oxnard was here speaking of Lucy's pelvic structure rather than a toe bone in *A. africanus.* Thus, within the context of an attempt to refute the humanlike characteristics of Lucy's pelvis in the course of referring to Oxnard's paper,

Parker contends: "Viewed one way, for example, the *pelvic bones* of australopithecines seem to be intermediate between man and ape. But merely viewing the bones from a different angle makes the specimen seem *as far distant from man as the apes*" (1983:122, emphasis added). Note how Parker has used phraseology nearly identical to that used by Oxnard. But, where Oxnard was speaking of an abstract *model* based on a comparison of *toe bones,* Parker creates the false impression that he was referring to "Lucy's" pelvis.

Incidentally, Parker's argument here is totally fallacious on its own merits. He seeks to deny a transitional status to Lucy by contending that, while it is true that her pelvis is humanlike when viewed from one angle, it is apelike when viewed from another, as if the apelike characteristics somehow canceled out the humanlike characteristics. Completely ignored is the obvious fact that we would expect a transitional form to be a combination of such traits. If, as Parker claims, Lucy was nothing but an ape, then her pelvis should be apelike regardless of the angle from which it is viewed.

Parker provides another form of the out-of-context quotation popular among "scientific" creationists, the practice of deleting words from cited material when those words contradict the point the creationist wishes to make. Parker writes: " 'Yet another view,' says Oxnard, 'might suggest that the fossil arose from the African apes via modern humans!' " (1983:122). When we turn to Oxnard's original paper, we discover that he actually said: "Yet another view *if assessed naively* might suggest that the fossil arose from the African apes via modern humans!" (1979:268, emphasis added). Parker deleted the qualifying words apparently to create the impression that Oxnard considers this to be an equally viable interpretation of the data. Here we have a case of apparently conscious deception by Parker.

Morris and Parker's book is proclaimed on the cover to be "suitable for public schools." Its shoddy, dishonest, and deceptive scholarship, of which I've given only a minuscule sampling, proclaims the inevitable results of adopting a zealous belief system that claims a monopoly on the truth. Although Parker pays repeated lip service to the critical spirit that motivates Oxnard and even suggests that such a critical attitude also be applied to creation "science," he consistently fails to apply such a critical attitude toward his own religiously motivated beliefs. Like most of his fellow "scientific" creationists, Parker utilizes deceptive tactics to attack any idea that challenges his own convictions.

Parker is cited as a consultant in the credits for "Origins: The Scientific Case for Creation" (Eden Films), a creationist film series currently making the rounds. The fifth film in the series, "The Origin of Mankind," finds Zuckerman and Oxnard once again being dishonestly cited in support of the contention that *Australopithecus* (including Lucy) was nothing more than an ape. In yet another demonstration of the mendacity that seems to be a prerequisite to membership in the ranks of "scientific" creationism,

practically every reference to *Australopithecus* in this film is illustrated with shots of *A. robustus,* the form of *Australopithecus* that is least humanlike and most apelike in its appearance.

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*Acts & Facts,* 1983, p. 8.

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# INDEX

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# Book Reviews

## Chronicle of a Phenomenon

*Mute Evidence.* By Ian Summers and Dan Kagan. Bantam Books, New York, 1984. 502 pages. $4.95.

Kathleen Ayers

IN 1967, SNIPPY, a horse, was reported by its owners to have been mutilated. I remember reading about it with all the interest any nine-year-old would have. It was fascinating to think that someone (or something) would come along and do that sort of thing to an innocent horse. Why? Who? The world began to speculate about it when a few years later mutilations of cows were reported. Some of this speculation was fruitful, but most of it was merely ludicrous.

*Mute Evidence* chronicles the spate of reported mutilations that occurred in the 1970s. Ian Summers and Dan Kagan have performed an exhaustive, thorough search for the truth of these reports, no matter how bizarre some of them were. The authors' attention to detail, the scope of their research, and the volume of facts they have accumulated are presented in a fairly orderly, comprehensive tome.

Criss-crossing the country, they tracked down sheriffs, cattle owners, veterinary pathologists, UFO buffs, newsletter publishers, and other assorted sources. They were scrupulously fair to everyone at first, no matter how far-out a person's ideas were. In fact, Kagan and Summers were so impartial, at first I wasn't sure which theory they believed.

But one by one, each theory that seemed to "explain" the reported mutilations was shot down: secret government operations, UFOs, cults, supernatural forces. The best debunker was an expert veterinary pathologist, Dr. John King, from Cornell. He gave the most elegant explanations of why cows can suddenly keel over, why they may *appear* to be mutilated, and why all the strange happenings that supposedly occur around a mutilated animal are not so strange after all.

*Kathleen Ayers writes from Bowie, Maryland.*

The wave of mutilations (referred to as "mutes" by followers of the cases) in Colorado in 1975 is reconstructed as a model for such waves in other states. In this model, we get to see the interaction of members of the press, public officials, and law officers. Politics, ego, self-advancement, and the desire to save face all contributed to the mess that was made of the situation. Lots of people could share the blame for correct information being withheld, for sensationalized reports in the newspapers, and for the overreaction of cattle owners, but no one accepted the blame. So, the reports of mutilated animals continued to build. The Colorado Bureau of Investigation looked into the matter. The Colorado State University Diagnostic Laboratory examined samples of tissue from these animals and reported that the cattle died from natural causes. But this did not satisfy the cattle owners, even though the number of dead cattle was well within the normal number that die every year. Why couldn't they just admit they were wrong? Instead, they advanced conspiracy theories and other outlandish ideas that made them look foolish.

Then outsiders—such as the UFO people—joined in, which led to more bizarre theories. People began to look for mutilations where none existed. Some members of the press were sloppy in their research, which only created more suspicion in the minds of cattle owners. Everyone blamed everyone else, and no one believed anyone else's theory. Luckily, the wave died down as the weather got colder. The next spring, and the years following, saw fewer reports.

The wave of reported mutilations in Colorado and other states finally ended around 1976, but it was followed by another spate of reports in New Mexico in the late 1970s. That one even triggered a federally funded official investigation by a former FBI agent that concluded that the mutilations were due to natural actions of predators and scavengers, amplified and exaggerated by credulous reports by the media, police officials, and ranchers feeding off one another's misinformation (*SI*, Fall 1980).

That is where the matter stands now, although believers maintain that the predator theory is naive. Perhaps there will be another wave of mutilation reports, and everyone will pick up the strands again. Maybe a few people will have learned from their past mistakes.

Overall, this is a most satisfying book. An index would have been helpful. To find specific names, I had to flip through the book repeatedly. The chapter titles offered little guidance.

If you were ever interested in the phenomenon of cattle "mutilations," read this book. Don't let the hype on the cover fool you. Summers and Kagan treat the subject with all the respect it deserves. •

# Some Recent Books

*Listing here does not preclude a more detailed review in a future issue.*

Bauer, Henry H. *Beyond Velikovsky: The History of a Public Controversy.* University of Illinois Press, Champaign, Ill. 1985. $21.95, 354 pp. A very

interesting and generally well-balanced exploration into the whole Velikovsky controversy and its meaning for other "trans-scientific" disputes. Although Bauer is a scientist, this is, appropriately enough for this controversy, more a sociological examination than a technical evaluation. Bauer is blunt in concluding that Velikovsky "was quite ignorant of science," and he dismisses as "nonsense" V's *explanation* of physical events. He blames V and his supporters for forcing the issue into the public (and therefore no longer scientific) arena early on. Yet he writes not altogether unsympathetically of Velikovsky. He has some unkind (and perhaps unfair) words for some of V's critics. Participants in and observers of the quarter-century-long Velikovsky controversy will find plenty to agree and disagree with here. See also Martin Gardner's column, this issue.

Binns, Ronald. *The Loch Ness Mystery Solved.* Prometheus Books, 700 E. Amherst St., Buffalo, N.Y. 14215, 1984, 228 pp., $18.95 cloth, $9.95, paper. First U.S. edition of this critical examination of the Loch Ness claims. Our reviewer (Winter 1983-84) said the book's strength lies in its excellent documentation and historical perspective. It dwells little on frauds and hoaxes but provides a host of unmysterious explanations about the eyewitness reports—influenced by the cultural milieu—that are the backbone of the Loch Ness "mystery."

Booth, John. *Psychic Paradoxes.* Ridgeway Press, 12032 Montecito Road, Los Alamitos, Calif. 90720. 1984. 240 pp. Limited Ed., $33.50 + $1.50 shipping. An intriguing and well-informed examination of the world of psychics from the viewpoint of a magician turned Unitarian clergyman and writer. Makes special efforts to sort truth from fiction and explore the multifarious ways and reasons people deceive themselves. Twenty chapters organized into sections on Theatrical Psi, Tricks of the Trade, The Psychic Connection, Spurious Phenomena, and Serious Psi Research. "In writing this book," says the author, "my hope was to make persons aware of the psychological ploys by which self-deception is cultivated in and by others and to show the methods by which seeming miracles are achieved by entertainers masking as legitimate possessors of psi powers."

Hall, Trevor H. *The Medium and the Scientist.* Prometheus Books, Buffalo, N.Y. 14215, 1984, 188 pp., $18.95. The remarkable story of Florence Cook and William Crookes told by British writer and historian. Crookes, an eminent British chemist, investigated and declared genuine the attendant spirit of Cook, a materializing medium. Hall sifts through the contemporary evidence and previously unknown documents and provides a thorough new evaluation of the Cook séances, the Crookes experiments, and the relationship between Cook and Crookes.

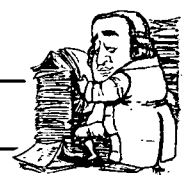
—*Kendrick Frazier*

Medawar, Peter. *Pluto's Republic.* Oxford University Press, New York, 1984, 351 pp., $8.95. The Nobel Prize winner's exploration of the netherworld of science in a series of collected essays deals more with lousy science than "parascience," but with opinionated, devastating wit he skewers IQ psychologists, psychoanalysts, anti-evolutionists, and others whose science does not meet his standards. The aphorisms and one-liners alone are worth the

price of admission, at least for the reader who enjoys erudite, upper-class British wit and exaggeration by understatement. "Aristotle's conception enriches or replaces scientific truth by truth of a higher kind, that which represents the testimony of a deeper and more privileged insight—a truth so lofty that, if nature does not conform to it, why then, so much the worse for nature." Writing about the laws of thermodynamics, he notes, " 'Information,' said Professor Norbert Wiener, in a passage more than usually full of negative entropy, 'is information, not matter or energy.' "

—*John R. Cole*

# Articles of Note



"Big Foot's Constituency" (editorial). *Nature,* 313:418, February 7, 1985. Lively commentary on reasons for the popular fascination with mythical creatures: Bigfoot, Yeti, the Loch Ness monster, and now even " 'Chessie,' the New World's answer to Nessie." Ascribes much of the attention to devotees with ability to raise money for expeditions to interesting places ("invariably equipped with cameras whose lens caps show a special propensity for remaining in place at critical moments") as well as to regional boosterism ("local pride and a nose for publicity").

Diamond, Jared. "In Quest of the Wild and Weird." *Discover,* March 1985, pp. 34-42. Insightful and balanced evaluation of cryptozoology, the quest for "hidden" or extinct large animals. The author, drawing on his own zoological research and discussions with local peoples in remote regions, is generally dubious. "I can only conclude that accounts initially suggestive of mermaids and dinosaurs in New Guinea—or anywhere else—invariably have mundane explanations: misunderstood questions, questions answered 'yes' in order to please the questioner, and natives amusing themselves at the expense of gullible Westerners." Yes, new species are regularly discovered. "The animals actually being found are, alas, bush league, mostly small creatures of little consequence to anyone other than specialists."

Elon, Amos. "A Hoax That Embarrassed the Experts." *Parade,* February 3, 1985, pp. 12-14. Report on the Modigliani sculpture hoax in Italy. A much-publicized 1984 discovery of several sculptured heads supposedly thrown away by the artist in a fit of depression in 1909 turned out to have been made by three college students and tossed, less than eight hours before they were found, into a canal being dredged. Nothing paranormal here, just an instructive view of the fallibility of experts certain in their knowledge. (Illustrious art critics had declared the heads authentic.)

Gardner, Martin. "666 and All That." *Discover,* February 1985, pp. 34-35. Report on the attempt of biblical numerologists to find the digits 666, Revelation's supposed symbol of the Antichrist, in the names of those they fear. Gardner

shows that 666 can be uncovered in almost anybody's name, "if you're willing to work a little at such mischief-making."

Golden, Fred. "Facing Up to Mars." *Discover,* April 1985, p. 92. "Skeptical Eye" column examines the recently resurrected claim that rock formations on Mars show a "face" and other relics of an ancient civilization. Quotes planetary scientists, who point out that the features have been known since Viking first orbited Mars in 1976 and that they are natural. Says Viking-team geologist Hal Masursky, "You can find 'faces' in any number of terrestrial mountain ranges, and I can show you better formed 'pyramids' in aerial photographs of Arizona."

Hövelmann, Gerd H. "Audiatur et Altera Pars. A Tribute to a Close Friend: Piet Hein Hoebens (1948-1984)." *European Journal of Parapsychology,* 5 (1984):191-202. A moving, well-informed, and detailed tribute to Piet Hoebens for his very special contributions to the responsible evaluation of parapsychology and to the mediation of misunderstandings between responsible parapsychologists and critics. Hoebens, a frequent contributor to the SKEPTICAL INQUIRER and a trusted colleague, died October 22, 1984, in Amsterdam at the age of 36.

Hogart, Simon. "The Ghostbusters." *The Observer* (London), November 25, 1984, pp. 21-22. Lengthy and lively report on the CSICOP conference at Stanford.

Ianna, Philip A., and Roger B. Culver. "Astrology and Cosmology" (Correspondence). *Nature,* 313:426, February 7, 1985. Strong response to critical review by H. J. Eysenck of their book *The Gemini Syndrome.* Eysenck (*Nature,* November 15, 1984) charged they neglected and underrated the "cosmobiology" of M. Gauquelin. The authors state why their view of Gauquelin's claims is considerably less positive than Eysenck's. A second letter, by Amardeo Sarma, criticizes the review for endorsing "superstition and pseudoscience."

Joyce, Christopher. "There's No Fool Like an Old Fool." *New Scientist,* January 17, 1985, pp. 30-31. Report on medical quackery in the U.S., keyed to the recent congressional report (*SI,* Winter 1984-85). Joyce finds it puzzling that popular quack remedies remain in the U.S.: "Rather than wither away in the shadow of blossoming medical science, the quacks simply adopt the new language of science. . . . The quacksalvers have also learned the quintessential American arts of persuasive advertising and of promotion by direct mail, targeting elderly groups and other easy marks."

Kolata, Gina. "Prestidigitator of Digits." *Science 85,* April 1985, pp. 66-72. Good profile of Persi Diaconis. World-renowned magician turned Stanford statistician, Diaconis can make a coin toss come up heads every time. This leads him to ponder philosophical questions and mathematical mysteries of randomness. Diaconis and colleagues have also determined the patterns that arise when a deck of cards is perfectly shuffled over and over again.

McCourt, Rick. "Walking on Fire." *Science 85,* March 1985, p. 84. Short report on the fad of firewalking. Gives emphasis to several physical explanations, including that of UCLA physicist Bernard Leikind, who shows that the heat capacity of coals is small and that they cool quickly.

Nickell, Joe, and John F. Fischer. "Reeser's Spontaneous Human Combustion Case Reconsidered." *Fate,* April 1985, pp. 64-68. Reexamination of a case in Florida in 1951 when Mary Reeser was said to have burned to death by spontaneous human combustion. Finds there was a lethal combination of

cigarette, flammable night clothing, and sleeping pills plus a large stuffed chair and a victim with high body fat that contributed to the burning. Also, certain other crucial details were wrong, misinterpreted, or missing from the popular reports. "When all the evidence in the Reeser case is weighed, we see no reason to invoke the so-called theory of spontaneous human combustion. . . . Instead, while the death had unusual features, it *is* understandable in scientific terms."

Ridpath, Ian. "A Flashlight in the Forest." *The Guardian* (U.K.), January 5, 1985. Journalistic investigation solving the case of a dramatic UFO report near an air force base in England.

Thomason, Sarah Grey. "Do You Remember Your Previous Life's Language in Your Present Reincarnation?" *American Speech,* 59, no. 4, Winter 1984, pp. 340-350. Fascinating paper by a University of Pittsburgh linguist. She addresses the claim by proponents of reincarnation that some people who produce fluent-sounding speech under hypnosis or when "speaking in tongues" are speaking a language used in an earlier life. She shows that "putative previous-life languages spoken by subjects under hypnosis are in fact fluent gibberish." She found a total lack of grammatical patterns and real words but a phonological structure, including natural-sounding intonation, based on the speaker's native language and any foreign languages he has been exposed to. She also made other investigations and concludes, ". . . the available evidence does not support the claim that anyone has ever spoken a real human language without being previously exposed to it in a systematic way in his or her current life."

Vulovic, Vladimir, and Richard E. Prange. "How Random Is a Coin Toss?" Paper presented at March meeting of American Physical Society, Baltimore, Md., March 25-29, 1985. University of Maryland physicists analyzed the process of a coin toss and found that the tossing process itself is not fundamentally random. "With sufficient care and practice, or with a precision mechanical tosser, the outcome of a coin toss can be predicted." (See also Kolata, on Diaconis, above.)

Weyrich, Noel. "Ghostbooster." *Philadelphia City Paper,* February 1-14, 1985, pp. 1, 4. Profile of Tam Mossman, a Prentice-Hall editor responsible for publication of *The Amityville Horror,* editor of its series of Seth Books (the supposed trance writing of an Elmira, N.Y., schoolteacher), and founder of a magazine (*Metapsychology*) "devoted to conversations with disembodied critters from our future." Only moderately skeptical, but filled with interesting information. The same issue contains "Out of the Closet at Last," by Ronnie Polaneczky, a look at who reads the outrageous *Weekly World News.*

—*Kendrick Frazier*

## Pamphlet Series

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published by the American Anthropological Association, 1703 New Hampshire Ave., N.W., Washington, D.C. 20009. Single copies free upon request (send stamped, addressed envelope). Additional copies 25 cents each for orders of less than 100, 20 cents each for orders of 100 or more. A first-rate and long-needed series of brochures prepared to address public misunderstandings about evolution fostered in large part by the creationist movement. Each is by an authoritative scholar. These attractive and handy brochures are clearly written and professional in tone. Each contains a list of suggested readings. A worthy publication effort by the AAA.

—*K.F.*

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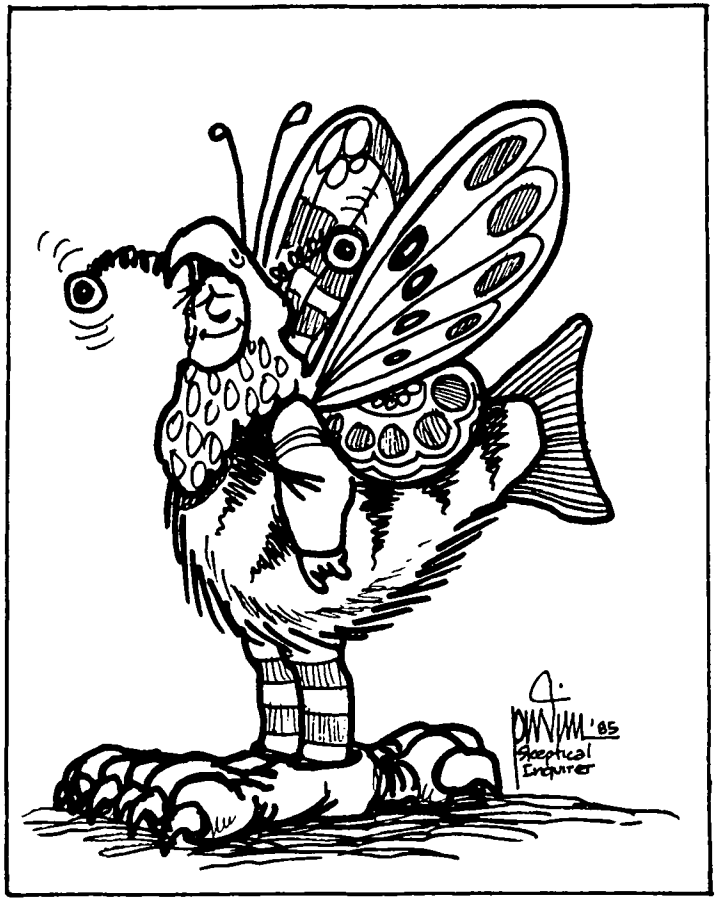


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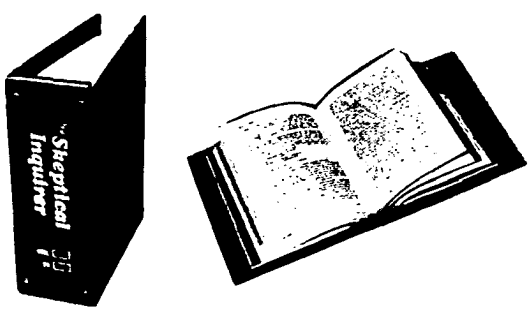


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# Follow-up

## Loch Ness Reanalysis: Rines Responds

WE WOULD LIKE to respond to the article by Razdan and Kielar (*SI*, Winter 1984-85) presenting criticism of some of the researches of members of our team at Loch Ness, to point out some of the misunderstandings and misstatements underlying the critique.

There is first an attempt to confuse an earlier, inconclusive 1971 sighting with a most definitive sighting in which Mrs. Rines also participated on July 23 of that year and which had nothing whatever to do with "dowsing." On this occasion, to the contrary, Mrs. Carey was drinking tea, not dowsing, and her husband made the chance initial observation of the hump. (See contemporaneous account published in Dinsdale, *Monster Hunt*, Acropolis Books, 1972, pp. x, xi.)

Next, there is a partial quote of Klein as to a particular sonar trace obtained in "first experiments" in September 1970, as probably being from three large fish. The authors, however, omitted the rest of the sentence, which says "or from three sections of a single larger moving object" (p. 33 of Ref. 4). There is also the omission of Klein's conclusion as to "large moving objects" detected in other tests (p. 35 and Fig. 6) and his distinguishing from "boat and diver traffic" and "bubbles caused by a ship propellor" (pp. 31, 33). The attempt to belittle other work of Klein not only reflects a lack of knowledge of Klein's sense of humor with "Kleinhenge," but also suppresses the fact that Dr. Ian Morrison of the University of Edinburgh and Dr. Edgerton, following a further Academy expedition, published the canal rock-dumping findings years before the authors, and that no one yet knows the interpretation of the deep-water targets found by Klein.

The authors then try to deprecate the August 8, 1972, overnight sonar results by providing startling "new" information that the equipment was suddenly lowered overboard at Mrs. Carey's light signal, that it was "free-swinging," and that the sonar echoes resulted from "several investigators [who] were rowing a large wooden fishing boat"—"the obvious characteristics of boat wakes." The authors, however, ignored the contemporaneous Loch Ness Investigation Bureau representative's account in Witchell's book (Ref. 12), clearly pointing out that the equipment had been continuously in the water at least since midnight. Further, that it was *after* the "big, black trace started to appear" that Bob Rines "and Jan Willums [of the Academy] got into *Fussy Hen* and we rowed as quickly and

quietly as we could back across to *Narwhal.* When we got back on board the echo was still there, slowly being etched out on the paper." And it was not until later when a "slight breeze got up" that "Narwhal started to swing round and we lost it." The further fact is that while there were floating experiments during the several successive evenings of the expedition, from at least 1:00 to 2:00 A.M. on August 8, 1972, the two boats were held in position in flat calm by the respective camera-strobe on a metal stand lowered to the bottom and the outwardly pointed sonar transducer lashed to a cinder block, then also lowered to the bottom at least 10 feet higher up on the ridge than the camera-strobe. This fixed position is also evident more particularly from the larger section of the sonar chart published in *Technology Review,* 79, no. 2 (December 1976):14, showing the constant, not free-swinging, permanent echoes from fixed near objects within about 5 to 10 feet. This is also all correctly illustrated in the contemporaneous drawing released by the Academy and LNI in November 1972, and then presented to the Institute of Electrical and Electronic Engineers meeting and the Museum of Science in Boston by Mr. Rines and Miss Holly Arnold of the LNI (see, also, Witchell, plate 43).

As if the twisting of historical facts is not enough, we wonder how the sonar intrusions of relatively constant range, almost 100 feet laterally from the sonar transducer, can be interpreted by any sonar "expert" as boat wakes moving toward the transducer boat. Significantly, however, the authors substituted their "informed" speculation for the contemporaneous records and analyses of leading sonar experts from Raytheon (the sonar manufacturer), MIT, Simrad (Norway), Hydroacoustics, Klein Associates, etc.

It is also almost unbelievable that the authors complain of the absence of a sonar echo from an "umbilical cable." The camera strobe equipment was self-powered, and there never was any such "umbilical cable" at all.

We are tempted to ask whether after hours of LNI–Academy monitoring, and over several successive nights with no results, Mrs. Carey at about 1:00 A.M. suddenly "dowsed" large moving echoes on the sonar chart and at the very same time placed objects in front of the Edgerton underwater strobe camera. Did she also "dowse" up salmon that were heard suddenly jumping about as the sonar chart showed fish racing away from the targets?

Having thus "destroyed" the soundness of the sonar results, the authors then repeat part of their "exposé" in *Discover* (September 1984) of the flipper pictures (which they do not apparently realize were correlated by time recordings with the sonar results), but this time with two significant changes. First, apparently as a result of Mr. Wyckoff's refutation, which the editor of *Discover* never printed, but a copy of which, we are told, was sent to SKEPTICAL INQUIRER, we no longer see the allegation of *Discover* that the computer scan was "retouched"; and we no longer see attributed to the Academy the sepia reproduction of the flipper picture that underlay the *Discover* article. Now, however, it is mildly stated that there are "discrepancies" between the published print of the film and a single filtered computer enhancement (one of many apparently unknown to the authors), which, of course, is hardly surprising. Again, however, the authors fail to disclose that at least one of the "computer enhancements," which they now describe as "apparently showing two large bodies," had been published by the Academy with no "discrepancies" whatsoever (even from their viewpoint). The authors also totally ignore Witchell's discussion (p. 132) of the contemporaneous appraisals of the *original* (not enhanced) elapsed-time 16mm film by Dr. Peters and Dr. Zug at the

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Smithsonian (then characterizing the shapes as like "the tail of the palmate newt"), of Dr. Sheals of the British Museum of National History ("passage of a large object . . . the films are genuine underwater photographs"), of Mr. Lyman of the New England Aquarium ("flipper"), or the unenhanced photo publication by *Time* magazine on November 20, 1972, which *Time* termed an "appendage"—all without benefit of a computer! It is curious that the editor of *Discover* at the time of their "exposé" had been earlier a member of the *Time* staff.

With regard to the 1975 underwater elapsed-time pictures, while Dick Raynor has certainly helped with equipment recovery over the years, and while we have at times recovered equipment from the bottom, the "head, neck and body" shots were recovered on June 20, 1975, during a week of successive overnight experiments and on a date when Mr. Raynor was not present, and where the gear was free-floating and not on the bottom. (This highlights the sloppiness of equating oral statements more than a dozen years after an event with actual fact, which could have been avoided by consulting the original notebooks and records). Had the authors understood the multiple sequence of elapsed-time underwater pictures published in the before-mentioned *Technology Review* (December 1976), Figs. C and E, and the calibration or reference pictures of the bottom (p. 30), or had they accepted the invitation extended by Messrs. Rines and Curtis of the Academy to study all the original materials and records and to discuss the same with the Academy team at Dr. Edgerton's MIT laboratory, they would have understood the unambiguous mid-water, not bottom, shots in many frames preceding and following these photographs C and E.

The final blow in the SKEPTICAL INQUIRER article is the misdescription of the 1976 experiments, results, and paper of Professor Edgerton and Mr. Wyckoff. The authors appear to imply that the Edgerton sonar array (EGG side-scan) was suspended from the Academy raft and was not "rigidly fixed underwater," and that the traces published in the *IEEE Spectrum* article (Ref. 3) were from "the wake of a boat." The facts are that this sonar transducer array in these experiments was not on any raft at all. In fact, the raft was not installed until July 4, four days after the exciting traces on June 30 and July 1, 1976! The sonar array to the contrary was fixed rigidly by a metal stand on the bottom near the shore at Temple Pier, pointing laterally outward into Urquhart Bay, as correctly described in the Edgerton-Wyckoff paper. The sonar traces shown in the article occurred at 10:44 P.M. on June 30, 1976, and at 5:15 A.M. the next day. Mr. Wyckoff personally rushed outdoors to the flat, calm lochside to ensure against artifact.

We should point out that it is not the authors who "discovered" that during the initial set-up of this equipment we had an artifact some days earlier in connection with an in-shore "wake of a boat," but the Academy expedition scribe, Dennis Meredith, published this fact seven years ago. What the authors are apparently attempting here is to confuse this early artifact with the later-obtained results published in the Edgerton-Wyckoff article.

The unbelievable assertion that "no calibration sonar traces were included in his paper for comparision" tells the story—we fear—that the authors either never studied the Edgerton-Wyckoff paper or are not skilled enough in side-scan or deep lateral beam sonar interpretation to know what they are reading.

In Fig. 3 of this article (Ref. 3), the published sonar chart distinctly shows the deliberate permanent *calibration echo* of the Academy cameras suspended 8 meters deep in 20 meters of water 55 meters distant from the fixed sonar transducer array, and of known dimensions. The calibration echo is labeled "camera"

and appears simultaneously with the large target (and we believe, animal) echoes detected from 80 to 110 meters further out from the cameras!

We are sorry to have to write this discrediting response.

*Robert Rines*
*Academy of Applied Science*
*Concord, N.H.*

*The following are listed as coauthors of Rines's reply: Harold E. Edgerton, Charles Wyckoff, Martin Klein, Robert Needleman, and Howard S. Curtis.*

## Loch Ness Reanalysis: Authors Reply

RINES'S RESPONSE suggests to us that he is attempting to obfuscate the main points raised in our SKEPTICAL INQUIRER article and in the September 1984 *Discover* "Skeptical Eye." Drawing attention to items with little or no relevance to our discussion will not help Rines extricate himself from the morass in which he and his colleagues now find themselves.

Nowhere was Rines able to deny the central issues of our article, that:

1. The events surrounding the August 8-9, 1972, Academy experiment were not adequately or accurately described in papers subsequently published by the investigators. Conclusions drawn from the data as presented by the Academy investigators should therefore be re-evaluated.

2. The computer-enhanced "flipper" photographs were significantly altered in a subjective manner prior to publication.

3. Upon close scrutiny, the various other Academy investigations mentioned in our paper contained significant procedural and/or interpretive errors rendering the results, at best, inconclusive.

As to the events surrounding the August 8-9, 1972, Academy experiment, in paragraph 4 of his response Rines unwittingly supports our contentions that the sonar transducer unit was not fixed to the bottom and that the sonar traces obtained that night were probably boat wakes. Here Rines quotes Witchell's book, stating "A slight breeze got up" and "Narwhal started to swing around and we lost it [sonar contact]." If the transducer was indeed fixed to the bottom, as illustrated in publications by the Academy members, this could never have occurred. Toward the end of paragraph 4 of his response, Rines introduces evidence from a sonar chart (published in the December 1976 *Technology Review*) that he claims shows "the constant, not free-swinging, permament echoes from fixed near objects within about 5 to 10 feet." Rines neglects to mention that these "constant . . . permanent echoes" shift, appear, and disappear on the sonar chart as the sonar transducer unit swung about underwater. Also in paragraph 4, Rines correctly quotes Witchell that it was *after* the "big, black trace started to appear" that Bob Rines "and Jan Willums [of the Academy] got into *Fussy Hen* and we

rowed as quickly and as quietly as we could back across to *Narwhal.*" Rines fails to mention, however (and it is clearly stated in Witchell's book), that the *Fussy Hen* was *first* rowed over the general path of the sonar beam by Peter Davies to pick up Rines and Willums. Given the ambiguous position of the sonar transducer, any disturbance caused by the camera/strobe boat could have initiated the sequence of events described in Witchell's book.

Rines's complaint in paragraph 5 again neglects the fact that the *Fussy Hen* was rowed *back* and *forth* between the boat tending the sonar and the boat tending the camera/strobe. That the sonar traces are of relatively constant range can be attributed to the fact that boat wakes close to the boat tending the sonar would probably not have fallen within the sonar's conical beam as it moved about underwater. Perhaps, if the leading sonar experts to whom Rines refers had been fully informed of the careless methods actually employed in obtaining the sonar traces, their opinions might have changed, as ours did.

In paragraph 6, Rines's objection to our complaint of the absence of sonar echo from the camera/strobe's umbilical cable is pure semantics. However the strobe was powered, it was, by his own admission (in paragraph 4), tethered to the boat. Such a tether should have shown up on the sonar chart as a straight line, if the published description of the 1972 set-up had been accurate.

Rines does not deny that the dowsing episode of August 8-9, 1972, took place as we described. It is only *now,* in paragraphs 2 and 7, and in the first sentence of paragraph 4, that he acknowledges dowsing was an element in the Academy's investigation. This fact was omitted in all published accounts of the findings of the AAS.

In paragraph 8, Rines does not deny, but attempts to trivialize, our claim that the JPL-enhanced photographs were significantly altered to give the impression of the flipperlike objects that appear in the published versions. Rines also implies that Wyckoff's letter (of which we have a copy), sent to the editor of Time-Life, refuted the claims made in the *Discover* column. In fact, Wyckoff's letter *supports* our contention that the JPL-enhanced photographs were further subjectively enhanced. We maintain that these alterations are of great importance in assessing the viability of Rines's data as evidence for large animals in Loch Ness.

As Rines must surely realize, the upper "flipper" picture, attributed to the Academy in our article, could not be printed in sepia because the SKEPTICAL INQUIRER is a black-and-white publication. The "flipper" picture printed in the SKEPTICAL INQUIRER is merely a black-and-white version of the same one that underlay the *Discover* article. Furthermore, the same photograph, or one indistinguishable from it, appears in the 1982 Academy of Applied Science, Report to the Membership, by Rines and edited by Curtis, clearly labled, "computer enhanced at Jet Propulsion Laboratories, California."

Also in paragraph 8, Rines attempts to give the mistaken impression that we now describe the third photograph from the *Technology Review* article as showing two bodies. By prefacing our remarks with "according to the Academy" (page 153 of our article), we intended to paraphrase the Academy's viewpoint, not our own. We saw no reason to discuss this photograph further. It is so ambiguous that we did not feel it worthy of comment. As to the "original (not enhanced)" underwater photographs, we should point out that in the March/April 1976 *Technology Review* article by Rines, Edgerton, Wyckoff, and Klein they state, "the [original, unenhanced] photographs obtained that night appear to be quite vague."

Regarding his paragraphs 8 and 9, Rines implies that only by consulting the Academy's original materials and records could we have been privy to details necessary for appropriate and accurate interpretation of the Academy's findings. Although one of us did initiate a meeting with Rines in Concord, N.H., on October 14, 1982, and with Edgerton at his MIT laboratory on October 15, 1982, on neither occasion were we offered the opportunity to review the Academy's original materials and records. We were led to believe, at that time, that the published reports accurately depicted the Academy's research and findings. After the publication of the *Discover* column, Rines telephoned us on August 8, 1984, at our lab in Rochester, N.Y. Nothing he said caused us to change our conclusions about the Academy's research. Rines suggested a meeting, but we felt it more appropriate to present our findings in a public forum.

As to Rines's paragraph 10, a careful reading of our article reveals that nowhere did we state that Edgerton's sonar was suspended from the Academy's raft. Also in regards to the 1976 Edgerton/Wyckoff experiment, and Rines's paragraph 12, even the most skilled interpreter of sonar data would find it impossible to obtain meaningful target-size information from the sonar charts in the IEEE article. The fixed echo labeled "camera" lacks any attributed reflectivity references for size calibration purposes. Rines does not dispute our statement that Edgerton and Wyckoff, erroneously, equated the width of their observed sonar targets with the actual trace thickness on their sonar chart recorder.

Regarding paragraph 11, Rines is the only one claiming that we "discovered" the boat wake episode. In fact, in our paper, it is clearly referenced (Ref. 43) that Meredith reported it. The boat wake incident clearly indicates Rines could have easily confused sonar traces from boat wakes with his *earlier* 1972 and 1975 contacts.

As to paragraph 3 of Rines's response, we were attempting to draw attention to the fact that Klein presented a more probable interpretation of the sonar traces in question. His statement that the trace was "probably from three large fish" is at least as worthy of notice as the assertion (subscribed to in the Academy's conclusions) that the trace emanated from "three sections of a single larger moving object."

Finally, Klein's *Technology Review* paper certainly does not appear to be written in a light or humorous vein. On the contrary, it presents sonar data taken at Loch Ness in a serious manner. The decision to publish an article ascribing his name to preliminary findings of "stone rings" was Klein's, not ours.

Published papers, purporting to be scientific in methodology and conclusions, should be complete, understandable, and verifiable. Our findings raise serious questions about the validity of Academy members' publications on Loch Ness, and Rines's response does nothing to clarify or resolve the discrepancies we have pointed out.

*Rikki Razdan*
*Alan Kielar*
*ISCAN, Inc.*
*Cambridge, Mass.*

*See also letter by Edward Kelly in From Our Readers section of this issue.*—ED.

# About the Cover

The cover illustration is a reproduction of a painting by Robert Tinney. The original painting is available for purchase from the artist. The outside dimensions are 20" x 28", including a 1½" border. The medium is Designer's Gouache, a permanent opaque water color. The artist's technique involves a brushed underpainting and air-brushed overpainting.

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*Mr. Tinney is the principle cover illustrator for BYTE magazine and has done cover illustrations for other national magazines and book publishers. Reproductions of his most popular covers are published in limited editions.*

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## Secrecy, Psi, and Pseudoscience

*Tales of high government silliness about secrecy are, of course, nothing new. . . . But it is more than just silliness when secrecy becomes a veil behind which travesties of public policy are committed. . . . Nobody knows exactly what the Central Intelligence Agency and the Department of Defense may be up to, which is why attention has been concentrated on the supposedly investigative reporting of one Ron McRae, who has just published an account of the U.S. Government's interest in parapsychology* (Mind Wars). [*See review in Spring 1984* SI.] *McRae's story in any case makes entertaining reading. We learn, for example, that the U.S. Navy hired 34 psychics to keep track of Soviet submarines, that the National Security Agency is applying clairvoyance to code-breaking and that worries over Soviet psychics peering into concrete silos led to the abandonment of the "race-track" basing mode for the MX missile. Only the untested arrogance that secrecy affords could permit such thinking to go on inside the government. Even a little dose of peer review (or scientific expertise) would do wonders to cure it.*

—In "Secret Science" (editorial). *Nature*, 310:352, Aug. 2, 1984.

# From Our Readers

## Editor's column: Exploring the fringes of science

While there is much that is admirable in Kendrick Frazier's long editorial on "Exploring the Fringes of Science" (*SI*, Winter 1984-85) I find his optimism, guarded though it is, unjustified and a bit disturbing.

Perhaps pseudoscience books do not sell as well as they did a decade ago, though most diet and self-help books are pseudoscience and are doing very nicely thank you. The hottest growth area in publishing today is religion—and what religion! I don't think trading von Däniken for Falwell is much of a bargain. The social upheavals of the sixties and early seventies are over, as Frazier says, and just look what we have: a president who has not only supported the teaching of creationism in public schools but apparently feels the biblical apocalypse is almost upon us. And he was reelected by an overwhelming majority of the American people. The Age of Aquarius is no longer with us, but is the Age of Fundamentalism an improvement?

What's disturbing about Frazier's editorial is its lack of proportion. Though he acknowledges the pernicious influence of creationism, he doesn't seem able to make a proper distinction between a creationist and a UFOlogist. This is a difficulty that some other *SI* writers seem to have. Belief in UFOs, spoon-bending, Bigfoot, even astrology, never represented a serious threat to rational thinking. They are at worst an irritation. Creationism, on the other hand, is part of a powerful and intolerant belief system, and one that has real and attainable political goals.

Doing battle with Russell Targ and *Fate* magazine may be amusing. It may even be useful. But it's also easy, and the victories are cheap and without much significance.

Don't pat yourself on the back for swatting mosquitoes when there are real tigers out there after your blood—and mine.

Daniel Cohen
Port Jervis, N.Y.

I find myself in the position of having to disagree with Mr. Frazier's statement that "there may be somewhat less public acceptance of pseudoscience now than ten years ago." The problem seems to be just the opposite: Much of the public accepts the paranormal tenets matter-of-factly, almost with a "So what?" attitude.

When the Air Force sent me to Europe, I'd hoped that I'd be leaving the cacophony of occultists far behind. Adding to this hope was the fact that the military's newspaper, *Stars and Stripes*, has no astrology column (although it often reports developments in the psychic world as "news").

Until I went to the on-base bookstore! The *National Enquirer*, other scandal sheets, and various books on astrology and horoscopes all clamored for the buyers' dollars. They didn't have

to try very hard.

The base broadcasting system regularly scheduled airings of "In Search of . . . ," and would sometimes use it as a filler when other programs failed to arrive.

When conversations turned to *The Amityville Horror* and *The Philadelphia Experiment,* the general consensus seemed to be meek acceptance of the versions presented by the authors and film producers.

I recently took a trip to Scotland, spending one night in Inverness. After I returned, my co-workers wanted to know whether I'd seen Nessie. The question of its existence was not a factor; it was as much reality to them as the Leaning Tower of Pisa.

Perhaps Frazier's reports of a trend toward skepticism are simply harbingers of an attitude that has not yet become prevalent among the U.S. military in Europe. If so, I apologize for my criticism and eagerly await the arrival of rationality.

Thank you for your efforts at injecting a-little sense into life.

Steve R. Graham
APO, N.Y.

I enjoyed your column "Exploring the Fringes of Science . . ." but I would like to comment on your statement "We ardently support each person's right to hold any belief he or she wishes." This seems at least misleading, if not completely false. After all, we *are* concerned over the alarming proliferation of belief in the paranormal, and much of the text of *SI* criticizes belief where logic and/or evidence is lacking. Such criticism, moreover, has a strong moral dimension because of the possible or actual consequences affecting our lives and well-being.

Now, if we criticize Jones for believing *x,* then we are saying that Jones ought not believe *x* (else we have no justification for being critical), i.e., that it is wrong for him to believe *x,* and therefore that he has *no right to believe x.*

Our nation was founded on a freedom of belief, just as it was founded on a freedom of action. But these expressions are inspirational and should be handled with care. From "You have a right to do as you please," it does not follow that you have a right to do just *anything;* and from "You have a right to believe as you please," it does not follow that you have a right to believe just *anything.*

William D. Gray
Dept. of Philosophy
San Diego City College
San Diego, Calif.

I was very pleased to read of *SI's* growth. This is a good sign for rational thinkers and thus for the future of society.

I was disturbed by two aspects of your editorial "Exploring the Fringes of Science: Critical Inquiries, Shifting Trends." Specifically, I was worried that you were taking to heart criticism that you have been "too harsh" and that you "don't take claims seriously enough." If anything, I fear that you have been far too charitable and forgiving in your treatment of various paranormal claims.

I have been planning to write you since I read the letter in the Fall '84 issue called "Science and Pseudoscience," by P. Naslin. I too was an ardent supporter of the SKEPTICAL INQUIRER from the moment I received my first issue. I too, however, have been finding myself growing bored by the rather staid and serious treatment that you and your authors give to truly wild and extraordinary claims.

I feel that you are faced with a somewhat difficult choice. On one hand, you desire (correctly, in my opinion) to be a reliable and quotable scientific reference source. On the other hand, you wish to reach as large an audience as possible. To do this, to reach and hold an ever wider audience, you must entertain.

I believe that the best way for you to entertain is to treat your subject mat-

ter with as much humor as possible. I do not believe that this approach would be in any way counter to the scientific principles that you wish to maintain. Quite the contrary: Levity as a result of a paper or report that utilized *reductio ad absurdum* to maximum effect would be both extraordinarily effective and highly entertaining. It is your humorous reports and anecdotes that have left the greatest impression. Further, reading and quoting such humorous bits to my friends and acquaintances has left us all rolling with laughter. Some of these individuals, I regret to say, formerly kept an "open mind" about Uri Geller and others of his breed. But after a few sessions of uproarious laughter they ceased to be potential "believers."

It is for this very reason that you are attacked so sharply when you use humor and *reductio ad absurdum* in your magazine. It is extraordinarily effective. The average individual simply cannot take paranormal claims seriously when they are exposed to the light of reason in such a way that they become impossible, absurd, and funny. Just imagine what the effect would be if the next time a "psychic detective" approached the police and the press he or she was greeted with uncontrollable mirth.

The second item that gives me cause for concern is your editorial policy toward religion. I believe that you are faced with a major contradiction and that to maintain the integrity of your journal you must grapple with it in no uncertain terms.

Religious belief almost universally contravenes physical laws, logical thought, and Occam's Razor—in other words, the very scientific principles and approach that the SKEPTICAL INQUIRER is dedicated to protect and uphold.

You have of course taken on many religious beliefs and causes, not the least of which has been creationism. By refusing to come to grips with the basic underlying issue of religion itself, however, you are tying your hands and, what's worse, you are not being logically consistent and true to the ideals and principles that the SKEPTICAL INQUIRER is dedicated to protect.

David Gerr
Throgs Neck, N.Y.

I'm getting a bit fed up with people who write to say that you should tone down your articles. If something is obviously absurd why not say so? To do less is *not* being intellectually honest. If these people need their debunking spoon-fed to them, then they're obviously not totally out of their paranormal closets.

I find honest, frank opinion refreshing. By toning down your articles you may give more credence to the phenomena that you're investigating than you intend. *That* would be dishonest, counterproductive, and against the standards that you have set for yourself and that we have come to expect. Keep up the good work!

August Berkshire
Minneapolis, Minn.

Keep up the good work and continue to stress what you expressed in your first-rate editorial, especially: "And the idea that all ideas are equal has begun to be replaced by a willingness to throw out those that don't work."

The magnitude of the task ahead came into sharp focus once again this morning as I received advertising material from *Reader's Digest* for a paranormal book. As long as a buck is to be made from the gullible, it would seem that spurious claims will never really die.

Walter Shropshire
Minister
Mount Carmel United
Methodist Church
Sunshine, Md.

The Winter 1984-85 *SI* is as entertaining and instructive as the publication always is, but in your editorial I found one contradiction in terms that rattles my

cage a little. You said that claims of the paranormal "may be getting wilder and even less credulous." Don't you mean "less credible"? *Webster's Third New International* defines "credulous" as "based upon or proceeding from credulity." That's about as accurate a description of the pottiness that passes for parapsychological evidence that I know. And I don't see anything either in your "Inquirer" or the one in the supermarket to suggest that claims—or claimants, for that matter—are getting any less credulous. Alas, if only they were.

Kathleen Stipek
Haines City, Fla.

*Kendrick Frazier replies:*

*A few comments. I share Daniel Cohen's views on the danger of creationism. The battle to prevent creationists and other fundamentalists from undermining science and education is still being fought and must be prosecuted forcefully. But at least the scientific community awakened to the threat and began effectively to respond, in books, testimony, and local action. Furthermore, an entire journal is published to critique creationist claims (*Creation/Evolution, *P.O. Box 146, Amherst Branch, Buffalo, N.Y. 14226), and the National Center for Science Education, Inc. was formed to coordinate local efforts in defense of evolution through state-by-state Committees of Correspondence and the* Creation/Evolution Newsletter *(Box 32, Concord College, Athens, W.Va. 24712.) As for Prof. Gray's concerns, I sympathize but I do not agree that our criticism of the beliefs of those with whom we disagree—no matter how strongly—is paramount to saying they have no* right *to those beliefs, folly though (in our view) they may be. At the same time, they have no right to force their beliefs on us or anyone else. And finally, yes, I did mean to write "less credible."*

## Loch Ness reappraisal

The patient and exact study ("Sonar and Photographic Searches for the Loch Ness Monster: A Reassessment," *SI*, Winter 1984-85) by Rikki Razdan and Alan Kielar is yet another step in proving spurious the Loch Ness "research" of Robert Rines and his Boston Academy of Applied Science. Razdan and Kielar are much more polite and scientific in their assessment of Rines and company's evidence of a monster in Loch Ness than were the photographic-enhancement experts I spoke with at the Jet Propulsion Laboratory of NASA in the fall of 1982.

In fact, photographic controversy notwithstanding, the entirety of Robert Rines's efforts in Loch Ness cannot support serious scrutiny, and one wonders if his "Academy of Applied Science" itself is any less mysterious than its research methods.

The Loch Ness media hoopla of 1976 was initially "sponsored" by a $25,000 "gift" to Rines's expedition by the *New York Times*. The money was supposed to ensure exclusive first-publication rights to films, videotapes, charts, and other data produced by the "scientists" involved in the search. To offset this meager expenditure the *Times* sold film rights to NBC, allowing them exclusive television rights to the story. At the time, John Noble Wilford, who was then Science News Director for the *Times*, wrote features on Loch Ness *ad nauseam*, and both the Academy of Applied Science and the *Times* promised "to confirm or upend once and for all" the existence of a monster in Loch Ness. However, long after Rines's fruitless undertaking, neither the *Times* nor the Academy ever commented on the existence of a Nessie.

One surmises that efforts at Loch Ness by American mass media from 1976 to 1980 were merely to generate news interest. In 1976 and 1977 alone, magazines like *Science News, Nature, Wildlife, Science Digest, Smithsonian, Technology Review* (MIT), *National Geographic*, and *Photography* ran feature articles on the joint venture of the

*New York Times* and the Academy of Applied Science. One may estimate that at least 40 million regular readers of these periodicals were exposed to the goings on at Loch Ness at this time. TV and newspapers also helped swell the number of Americans who expected something extraordinary to be pulled from the loch at any moment. And the respectable *Smithsonian* magazine was gullible enough—along with other periodicals—to pay for and print the Academy's questionable flipper photos. But *National Geographic* experts were unconvinced of the authenticity of the photos and turned them down. William S. Ellis, an editor who wrote the Loch Ness feature for the *Geographic*, told me he recalled that the asking price of the Academy for photo privileges was around $50,000.

Of course nothing was found in Loch Ness in 1976, 1977, or 1978. However, Rines and his Academy were not finished. Rines proceeded with his infamous "dolphin experiment," wherein he proposed to dump two dolphins equipped with miniature cameras into Loch Ness. The trained dolphins would swim around, photograph Nessie, and bring the exposures to Rines and company at lochside—no matter that dolphins are salt water mammals and Loch Ness fresh water! Don't ask how the cameras would be triggered; suffice it to say that one dolphin (on the endangered species list at that) died while "training" for the expedition and the experiment was abandoned. Of course the media did not sensationalize this grim event as they had the earlier stages of the search. American journalism rarely follows up, even on the very news it creates.

In any case, facts about the Academy of Applied Science itself might be surprising. When I visited it in Boston, after making an appointment, I was not allowed in, for the doorman told me it was Robert Rines's private apartment. What is the Academy's purpose? What does the Academy do when it isn't pursuing Nessie? Don't ask the *New York Times* or NBC.

To prove the flipper photos doubtful after so long is a sincere triumph; but surely such questioning should not seem tentative. If one examines Rines's and his Academy's efforts in Loch Ness, firm conclusions may be drawn that indicate little application of the kind of scientific effort that would be sustained by real scientists—that is, qualified investigators who would not attempt to profit by sensationalizing such an investigation.

Edward Kelly
Prof. of English
and Journalism
SUNY College at Oneonta
Oneonta, N.Y.

*See also exchange between Rines and Razdan and Kielar, "Follow-Up," this issue.*—ED.

I'd like to add one footnote to Razdan and Kielar's excellent article. On the subject of the different versions of the famous "flipper" photographs, the article states: "We feel this discrepancy is very important because it was these flipper photographs that led Sir Peter Scott to assign a zoological classification to the creature, *Nessiteras Rhombopteryx* (Ness Beast with the Diamond Shaped Fin)."

I believe that Sir Peter undertook his task of zoological classification with tongue firmly in cheek. The letters in NESSITERAS RHOMBOPTERYX are an anagram for MONSTER HOAX BY SIR PETER S.

David Healy
Manhasset, N.Y.

*In fairness we should add, as Robert Rines pointed out when this anagram was first reported in 1975, that the same letters can also spell, "Yes, Both Pix Are Monsters. R." In fact, in an article on this controversy in* Technology Review *(March/April 1976), all six of the article's subheads were anagrams of the proposed name.*—ED.

## Rhine and the Levy Affair

In the Winter 1984-85 *SI*, you summarize (pp. 182-183) some material I published in *Fate* magazine about J. B. Rhine's handling of the Levy affair. This material stemmed from 1974, when Rhine discovered that [W. J. Levy] his director of research (at the Institute for Parapsychology) was falsifying his data. The summary you published is extremely inaccurate and misleading. You say: "Rogo reveals that Rhine failed to listen to colleagues' suspicions that something was amiss and that later, when the fraud was apparent, made a series of attempts to keep it from becoming known." I said no such thing.

While it is my position that Rhine was hesitant to fully explore hints that Levy was falsifying his research, no attempts were ever made by Rhine to hide the exposure once it was proved. Rhine took pains to contact other researchers following up on the Levy work. My major concern was with Rhine's conduct during the aftermath of the scandal. Some researchers at the time, including myself, felt that Rhine was less than open about the fact that replications of the Levy work were failing at the Institute and that he tried to downplay the fact. There was also some reluctance on Rhine's part to explore the possible extent of the Levy fraud, though his staff threw themselves into this re-analysis with commendable zeal.

My position is that Rhine made some poor judgments during this famous crisis, not—as you imply—that a Watergate was going on at the Institute.

D. Scott Rogo
Northridge, Calif.

*The Editor responds:*

*We maintain that our two-sentence note was an accurate summary of Rogo's letter to* Fate, *and we could excerpt quotes to demonstrate that. Rogo's claims in* Fate, *however, are in dispute. A separate letter to us (not for publication) from Richard Broughton, of the Foundation for Research on the Nature of Man (formerly Rhine's Institute for Parapsychology), stated that the evidence Rogo cited was nonexistent and said Rogo was publishing a retraction in* Fate's *April issue. This letter, in which Rogo states he wishes "to clarify what I originally wrote," did appear in the April 1985 issue of* Fate, *pp. 113-114.*

*Following Broughton's communication, we received a second letter from Rogo, printed below. We will follow this controversy with a certain detached interest.*

I have been sent a copy of the letter that Dr. Broughton sent to you. Since this letter severely misrepresents the current status of the Levy matter, I feel a reply is necessary.

In no sense is my letter to *Fate* a "retraction of my claims." It is a letter of clarification in which I throw added light on the Rhine/Levy matter. I do formally retract the statement that Rhine bowed to outside pressure to fire Levy and do acknowledge that Levy's firing came shortly after Rhine learned of his fraud. What I do *not* retract is that Rhine did not fire Levy on the spot, that some sort of idea to keep Levy on after giving him a leave of absence was considered for a short time, and that he tried to interfere with free reporting of the fact that the Levy work was not replicating.

I also point out in that letter, in deference to FRNM, that the rumors of Levy's fraud before the exposure were probably not backed by hard enough evidence to warrant Rhine's immediate action. As I said, this is a letter of clarification, and neither *Fate* nor I approve of it being called a "retraction" except in the most limited sense.

This matter was brought before the Parapsychological Association council in December, and they urged me to publish this clarification and even suggested some of the wording. Despite a request from FRNM, the council would not act on their urging that I be offi-

cially censured for what I wrote. Dr. Broughton's claim that there was only nonexistent and self-serving testimony to support my views is contradicted by the written evidence provided to the council both by me and by them.

I do think, however, that the clarifications to be published in *Fate* should be brought to the attention of your readers. I am also concerned about the fact that your summary of what I wrote in *Fate,* as I have already written to you, misrepresented my views. What should be said in summary is that Rhine made some poor decisions regarding the Levy matter, not that he acted unethically or in a deceitful manner. He merely acted the way he always did—stubbornly and uncircumspectly—after having made up his mind on his course of action.

D. Scott Rogo

## Animal language claims

Michael Dennett, in his "Final Interview with Milbourne Christopher" (*SI,* Winter 1984-85), enumerates a wide variety of topics investigated by the late magician. By way of amplification, may I note that Milbourne Christopher, in his splendid *ESP, Seers and Psychics* (Crowell, 1970), also scrutinized, with a sharp critical eye—although, as Dennett notes, ever gently in tone and spirit—the "talking animals" illusion and craze.

Concerned chiefly with some domesticated mammals (horses, pigs, dogs) and birds (e.g., geese), his well-informed chapter, "ESP in Animals," was drafted less than a decade after loquacious dolphins surfaced but before these marine mammals removed their wetsuits and came ashore in the guise of chattering chimpanzees and gabbling gorillas. He would have been amused but hardly, I feel certain, surprised to register that, while the scope of the species implicated has spread beyond the familiar to the alien—a category that includes even elephants (cf. *SI,* 8:110) and harbor seals, woodpeckers and African tortoises—the logic and rhetoric of the reported researches have remained, as ever, disturbingly analogous to that used by parapsychologists.

For this reason alone, his insistence that "qualified magicians" be employed as "observers" where extraordinary claims are put forward should be assiduously applied in this relatively neglected domain of fringe-psychology, which features animals alleged to be language-inculcated, but which, thus far, have invariably turned out to be reincarnations of Clever Hans.

Thomas A. Sebeok
Indiana University
Bloomington, Ind.

## Medical quackery

Since *SI* is one of my favorite magazines, it is especially discouraging when the editor shows a pronounced lack of skepticism on a topic of big money and strong beliefs—quackery. Your favorable review (Winter 1984-85) of the congressional report "Quackery: A $10 Billion Scandal" not only supported the facts, with which I have little dispute, but also supported the proposed solutions, which are not scientific but political.

You have failed to discriminate between exposing foolish mistakes and advocating criminal penalties to suppress them. Although alcohol consumption is provably harmful in many ways, the national prohibition of alcohol was found to be a serious error. Many quack remedies are likewise provably harmful but advocacy of their prohibition is unscientific and unwarranted.

Harry Reid
Safety Harbor, Fla.

Your editor rails against medical quackery in America today. Once again the SKEPTICAL INQUIRER has extrapolated its opposition to the anti-physical-science movement (astrology, creationism, ESP, etc.) to the realm of

anti-establishment medicine, equating in the process the field of medicine with the disciplines of physics and astronomy, as if they equally laid claims to scientific fastidiousness and credibility.

But, in reality, the field of established medicine in America today is shot through with questionable and unscientific procedures, which one rarely finds in the physical sciences and which lead any serious observer to wonder where the majority of quacks can be found—outside, or *within,* the medical profession!

David Solan
Norwalk, Conn.

## Unit on scientific fallacies

As a science educator—mostly at the junior-senior high-school level—I've encountered in my students on numerous occasions scientific misconcepts, superstitions, and beliefs in the pseudoscientific and the paranormal that I think mirror a nationwide problem that doesn't speak well for science education in our public schools.

Three years ago I began developing a unit titled "Scientific Fakes, Frauds, and Fallacies" for use in my eighth-grade science class. This unit is based on a list of topics gleaned from *SI* and other sources.

The unit begins with an investigation of how scientists determine truth, which includes scientific methods of problem-solving, critical thinking, and development of skeptical attitudes. We then attempt to apply these skills to selected items from the list.

The unit has developed into a format of lecture-discussion, with class demonstrations and activities to illustrate and clarify points and concepts, with a primary goal of trying to explain all things by normal means. We also try to design experiments to test some of the topics or to research topics that don't lend themselves to our level of demonstrations.

The purpose of my letter is to ask *SI* readers for help. I would appreciate suggestions of topics to be explored, possible demonstrations, experiments, or activities suitable for eighth grade, and ideas for dealing with controversial topics.

On request, I will send a copy of my topics list, a set of my objectives, and brief description of the demonstrations and activities I use. I'm planning on eventually submitting the unit to my state department of education and other sources for possible use by other teachers. I truly need expert help in refining it and am looking forward to any and all responses.

Patrick Wilkinson
Platte Valley Jr.-Sr. High
Kersey, CO 80644

## Astrology in Quebec

One of the most famous astrologers in Montreal is Huguette Hirsig, who appears frequently on TV, on the radio, and in the newspapers.

In December 1984, I came across her book entitled *Prévisions astrologiques modailes 1984* ("World Astrological Predictions for 1984") published at the end of 1983, and I decided to verify her predictions for 1984 concerning Québec and the United States. I reduced her text to 30 distinct ideas. Then I met individually with three experts with different political affiliations: a historian, a professor of economics, and an accountant.

As far as possible, the experts were in a "blind" condition. First, they did not know they were judging an astrologer because I told them they were judging an expert in futurology, referred to during the interview as "Mr. X." Second, before presenting each statement, I asked the expert one or two questions that would make him express clearly his perception of the subject. Only after that did I read Mr. X's statement and ask for a judgment.

Here is a summary of the judgments of the three experts for the 30 statements: 6 of the statements were

judged to be banalities or generalities highly probable or impossible to verify; 9 were judged totally false; 11 rather false; 2 neither false nor true, 1 rather true, and 1 totally true. Only 2 successes out of 30 attempts—a success rate of 6.7 percent.

Guy Châtillon, Chairman
Dept. of Mathematics
and Computer Sciences
Université du Québec
Trois-Rivières, Québec
Canada

## Paranormal critiques in Pittsburgh

The Paranormal Investigating Committee of Pittsburgh (PICP) is now in its third year. Our group includes many university professors, mostly in psychology, and others with academic interests. I am a "mentalist" and a magician and act as chairman. We are still offering a prize of $5,000 to anyone for a valid demonstration of a paranormal claim. To date, we have had 40 claimants, all unsuccessful.

PICP is active as an educational resource in our area and tries to help fight the battle against pseudoscience on radio, on television, and in the press. We try to offer an alternative viewpoint to a largely misinformed public that doesn't often have the opportunity to hear responsible voices from the scientific community.

PICP would like to thank the CSICOP Fellows, as I'm sure would all the local groups, for their support. We specifically would like to single out Paul Kurtz and James Randi for their guidance and wisdom. We also have the "good luck" of having Dr. Donald McBurney, professor of psychology at the University of Pittsburgh, as a colleague. His contributions to our work are invaluable.

Richard Busch, Chairman
Paranormal Investigating
Committee of Pittsburgh
Pittsburgh, Pa.

## Rich, famous, and privileged

I wish to report that the media have again presented only one side of the story. On November 14, 1984, at 7:00 P.M., KMBC-TV in Kansas City, Missouri, aired "Lifestyles of the Rich and Famous." This program showed all of the houses and property Uri Geller has been able to buy with the money he has made giving lectures. This program presented an example of Uri's "ability." He bent a key belonging to the program's producer. No disclaimer was given nor any doubts expressed.

It is interesting that this same program included Arthur Jones, inventor of the Nautilus exercise equipment. Jones made several statements with which some people would disagree. Yet "Lifestyles of the Rich and Famous" felt obligated to run a disclaimer for the Jones segment but not for the Uri Geller interview.

Stephen Chappell
Columbia, Mo.

## Uri award for BBC?

I read your report on the Koestler bequest (Fall 1984), which, by accepting, the great and ancient University of Edinburgh has disgraced itself. It may amuse your readers to hear the story that is circulating here about the professorship: Rumor has it that the post will be awarded to Koestler himself but that the salary will be paid to the medium who transmits his lucubrations.

More seriously, I regret that I must nominate BBC-TV for a Uri award. A completely uncritical program featuring the "medium" Mary Stokes was recently broadcast, preceded by equally uncritical puffing in the corporation's weekly "Radio Times." O tempora! O mores!

M. Hammerton, Head
Dept. of Psychology
Univ. of Newcastle
Upon Tyne
Newcastle Upon Tyne
England

## Saint Jonathan Swift

I would like to say thanks to the people who bring me the SKEPTICAL INQUIRER, which publication gives monthly reminders that I am not alone.

Perhaps we should adopt a patron saint of skeptics. I nominate Jonathan Swift for the honor, based on his "Bickerstaff Papers." Here's what the *Encyclopaedia Britannica,* 14th ed., has to say about Swift in its entry on Astrology: "In England Swift may fairly claim the credit of having given the death-blow to astrology by his famous *Prediction for the Year 1708, by Isaac Bickerstaff, Esq."*

Murray Projector
Claremont, Calif.

## Local Organizations

(groups with aims similar to CSICOP's)

**Arizona**

Tucson Skeptical Society (TUSKS), Ken Morse, 2508 E. 23rd St., Tucson, AZ 85713.

**California**

Bay Area Skeptics, Robert A. Steiner, Chairman, Box 659, El Cerrito, CA 94530.

Southern California Skeptics, Al Seckel, Chairperson, P.O. Box 7000-39, Redondo Beach, CA 90277.

**Colorado**

Colorado Organization for a Rational Alternative to Pseudoscience (CO-RAP), Béla Scheiber, Director, P.O. Box 7277, Boulder, CO 80306.

**Idaho** (see Oregon-Idaho)

**Minnesota**

Minnesota Skeptics, Robert W. McCoy, 549 Turnpike Rd., Golden Valley, MN 55416.

**New York**

New York Committee for Skeptical Inquiry (NYCSI), Terence Hines, 51 Westchester Ave., Thornwood, NY 10594.

**Ohio**

South Shore Skeptics, Page Stephens, 1346 W. 64th St., Cleveland, OH 44102.

**Oregon-Idaho**

Northwest Skeptics, John Merrell, Oregon-Idaho Coordinator, P.O. Box 5027, Beaverton, OR 97007.

**Pennsylvania**

Paranormal Investigating Committee of Pittsburgh (PICP), Richard Busch, Chairman, 5841 Morrowfield Ave., #302, Pittsburgh, PA 15217.

**Texas**

Austin Society to Oppose Pseudoscience (A-STOP), W. Rory Coker, President, P.O. Box 3446, Austin, TX 78764.

Dallas Society to Oppose Pseudoscience (D-STOP), James P. Smith, Science Div. of Brookhaven College, Dallas, TX 75234.

Houston Society to Oppose Pseudoscience (H-STOP), Steven D. Schafersman, Chairman, P.O. Box 541314, Houston, TX 77254.

**Washington**

Northwest Skeptics, Michael R. Dennett, Chairman, Washington Coordinator, P.O. Box 70191, Seattle, WA 98107.

**West Virginia**

Committee for Research, Education, and Science Over Nonsense (REASON), Steven Cody, Chairperson, Dept. of Psychology, Marshall University, Huntington, WV 25701.

# The Committee for the Scientific Investigation of Claims of the Paranormal

**Paul Kurtz,** Chairman

## Scientific and Technical Consultants

**William Sims Bainbridge,** professor of sociology, University of Washington, Seattle. **Gary Bauslaugh,** dean of technical and academic education and professor of chemistry, Malaspina College, Nanaimo, British Columbia, Canada. **Richard E. Berendzen,** professor of astronomy, president, American University, Washington, D.C. **Barry L. Beyerstein,** professor of psychology, Simon Fraser University, Burnaby, British Columbia, Canada. **Richard Busch,** musician and magician, Pittsburgh, Pa. **Charles J. Cazeau,** associate professor of geological sciences, SUNY, Buffalo. **J. Dath,** professor of engineering, Ecole Royale Militaire, Brussels, Belgium. **Sid Deutsch,** professor of bioengineering, Rutgers Medical School. **J. Dommanget,** astronomer, Royale Observatory, Brussels, Belgium. **Natham J. Duker,** assistant professor of pathology, Temple University. **Frederic A. Friedel,** philosopher, Hamburg, West Germany. **Robert E. Funk,** anthropologist, New York State Museum & Science Service. **Laurie Godfrey,** anthropologist, University of Massachusetts. **Donald Goldsmith,** astronomer; president, Interstellar Media. **Norman Guttman,** professor of psychology, Duke University. **Clyde F. Herreid,** professor of biology, SUNY, Buffalo. **I. W. Kelly,** professor of psychology, University of Saskatchewan. **Richard H. Lange,** chief of nuclear medicine, Ellis Hospital, Schenectady, New York. **Gerald A. Larue,** professor of biblical history and archaeology, University of So. California. **Joel A. Moskowitz,** director of medical psychiatry, Calabasas Mental Health Services, Los Angeles. **Joe Nickell,** technical writing instructor, University of Kentucky. **Robert B. Painter,** professor of microbiology, School of Medicine, University of California. **John W. Patterson,** professor of materials science and engineering, Iowa State University. **Steven Pinker,** assistant professor of psychology, MIT. **James Pomerantz,** professor of psychology, SUNY, Buffalo. **Daisie Radner,** professor of philosophy, SUNY, Buffalo. **Michael Radner,** professor of philosophy, McMaster University, Hamilton, Ontario, Canada. **Robert H. Romer,** professor of physics, Amherst College. **Milton A. Rothman,** professor of physics, Trenton State College. **Karl Sabbagh,** journalist, Richmond, Surrey, England. **Robert J. Samp,** assistant professor of education and medicine, University of Wisconsin-Madison. **Steven D. Schafersman,** geologist, Rice University, Houston. **Stuart D. Scott, Jr.,** associate professor of anthropology, SUNY, Buffalo. **Erwin M. Segal,** professor of psychology, SUNY, Buffalo. **Elie A. Shneour,** biochemist; president, Biosystems Assoc., Ltd., La Jolla, California. **Steven N. Shore,** astronomer, Space Telescope Science Institute, Baltimore. **Barry Singer,** psychologist, Seal Beach, Calif. **Douglas Stalker,** associate professor of philosophy, University of Delaware. **Gordon Stein,** physiologist, author; editor of the *American Rationalist*. **Waclaw Szybalski,** professor, McArdle Laboratory, University of Wisconsin-Madison. **Ernest H. Taves,** psychoanalyst, Cambridge, Massachusetts.

## Subcommittees

**Astrology Subcommittee:** Chairman, I. W. Kelly, Dept. of Educational Psychology, University of Saskatchewan, Saskatoon, Saskatchewan S7N 0W0, Canada.

**Education Subcommittee:** Co-chairmen, James E. Alcock, Glendon College, York University, 2275 Bayville Ave., Toronto, and John R. Cole, CSICOP, Box 229, Buffalo, NY 14215-0229.

**Paranormal Health Claims Subcommittee:** Co-chairmen, William Jarvis, Chairman, Department of Public Health Science, School of Allied Health Professionals, Loma Linda University, Loma Linda, CA 93350, and Stephen Barrett, M.D., 842 Hamilton Mall, Allentown, PA 18101.

**Parapsychology Subcommittee:** Chairman, Ray Hyman, Psychology Dept., Univ. of Oregon, Eugene, OR 97402.

**UFO Subcommittee:** Chairman, Philip J. Klass, 404 "N" Street S.W., Washington, D.C. 20024.

## International Committees (partial list)

**Australia:** Mark Plummer, G.P.O. Box 1555 P. Melbourne 3001; Dick Smith, P.O. Box 321, North Ryde, N.W.S. 2113; Stewart Nicol, Physiology Dept., Univ. of Tasmania, Box 252C, GPO Hobart 7001. **Belgium:** J. Dommanget, Observatoire Royal de Belgique, Avenue Circulaire 3, B-1180 Brussels. **Canada:** James E. Alcock (chairman), Glendon College, York University, 2275 Bayville Ave., Toronto; Henry Gordon (media consultant), Box 505, Postal Station Z, Toronto M5N 2Z6. **Ecuador:** P. Schenkel, Casilla 6064 C.C.I., Quinot. **France:** Maurice Gross and Yves Galifret, Comité Français pour l'Etude des Phénomènes Paranormaux, 16 Rue de l'Ecole Polytechnique, Paris 5. **Great Britain:** Michael J. Hutchinson, 10 Crescent View, Loughton, Essex. **Italy:** Cesare Baj, Newton, Pigreco S.R.l., Via Volta 35, 22100 Como. **Mexico:** Mario Mendez-Acosta, Apartado Postal 19-546, Mexico 03900, D.F. **New Zealand:** David Marks, University of Otago, Dunedin. **Norway:** Jan S. Krogh, Norwegian Institute of Scientific Research and Enlightenment, P.O. Box 990, N-9401, Harstad. **Sweden:** Sven Ove Hansson, Box 185, 101 22, Stockholm 1.

## The Committee for the Scientific Investigation of Claims of the Paranormal

The Committee for the Scientific Investigation of Claims of the Paranormal attempts to encourage the critical investigation of paranormal and fringe-science claims from a responsible, scientific point of view and to disseminate factual information about the results of such inquiries to the scientific community and the public. To carry out these objectives the Committee:

- Maintains a network of people interested in critically examining claims of the paranormal.
- Prepares bibliographies of published materials that carefully examine such claims.
- Encourages and commissions research by objective and impartial inquirer in areas where it is needed.
- Convenes conferences and meetings.
- Publishes articles, monographs, and books that examine claims of the paranormal.
- Does not reject claims on a priori grounds, antecedent to inquiry, but rather examines them objectively and carefully.

The Committee is a nonprofit scientific and educational organization. THE SKEPTICAL INQUIRER is its official journal.